KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

LONDON: FRIDAY, APRIL 14, 1882.

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*** Our Exchange and Sixpenny Sale Columns appear on Page VII.—in our advertising columns this week.

THE GLORIES OF THE STAR-LIT HEAVENS.

By R. A. Proctor.

ON a dark, clear night,
When all the stars shine,
And the immeasurable heavens
Break open to their highest,

the glories of the stellar depths seem revealed in their fullest splendour. Yet how small a portion is seen. "These are but a part of God's ways; they utter but a whisper of His Glory." If the eye could gain gradually in light-gathering power, until it attained something like the range of the great gauging telescopes of the Herschels, how utterly would what we see now seem lost in the inconceivable glories thus gradually unfolded. Even the revelations of the telescope, save as they appeal to the mind's eye, would be as nothing to the splendid scene revealed, when within the spaces which now show black between the familiar stars of our constellations, thousands of brilliant orbs would be revealed. The milky luminosity of the Galaxy would be seen aglow with millions of suns, its richer portions blazing so resplendently that no eye could bear to gaze long upon the wondrous display. But with every increase of power more and more myriads of stars would break into view, until at last the scene would be unbearable in its splendour. The eye would seek for darkness as for rest. The mind would ask for a scene less oppressive in the magnificence of its inner meaning; for even as seen, wonderful though the display would be, the glorious scene would scarce express the millionth part of its real nature, as recognised by a mind conscious that each point of light was a sun like ours, each sun the centre of a scheme of worlds such as that globe on which we "live and move and have our being."

Who shall pretend to picture a scene so glorious? If the electric light could be applied to illumine fifty million lamps over the surface of a black domed vault, and those lamps were here gathered in rich clustering groups, there strewn more sparsely, after the way in which the stars are spread over the vault of heaven, something like the grandeur of the scene which we have imagined would be realised—but no human hands could ever produce such an exhibition of celestial imagery. As for maps, it is obviously impossible by any maps which could be drawn, no matter what their scale or plan, to present anything even approaching to a correct picture of the heavenly host. There is no way even of showing their numerical wealth in a single picture.

Take, for example, the chart of 324,198 stars which I drew eight years ago, of which a portion is roughly presented in the illustrative map. Here the points and discs representing stars congregate together so closely, in places, that there is no room for more to be shown, and those which are shown are but inadequately presented. Yet what does this chart show of the heavenly host, regarded merely in their numerical aspect? The stars here charted are only those which can be seen, or rather only those which have been seen, with a telescope $2\frac{3}{4}$ inches in aperture, such a telescope as can be seen in every optician's window. I say only those which *have* been seen, because I know from my own observations that Argelander and his assistant observers, had they cared to turn their telescope to the heavens only on the darkest and clearest nights, could certainly have seen and charted half-a-million stars at least, instead of the 324,000 which they have actually included in their survey of the Northern heavens. In the Southern heavens at least as many could be seen. A million stars within the range of a telescope absolutely insignificant compared with the gauging telescopes of the Herschels, which again in light-gathering power were feeble compared with the mighty Parsonstown reflector!

Utterly hopeless would it be to attempt to delineate the stellar host within the range of these noble instruments, when already we see the method of charting fail us for the work of the puny tube which Argelander employed. Yet how impressive is the scene roughly depicted in our chart! Each one of the points there shown represents a sun, and tells us therefore of a solar system, of a system in which such a globe as this earth would be but as a point, and regions exceeding in extent the mightiest kingdoms over which the monarchs of earth have ruled would be utterly as nothing.

When we pass onward from these glories to the vaster glories revealed by more powerful telescopes, we seem to lose ourselves in the contemplation of the mysteries of infinite space, infinite power, infinite wisdom. Yet it is not till we have learned to look on all that the telescope reveals as in its turn *nothing* compared with the real universe, that we have rightly learned the lessons which the heavens teach, so far, at least, as it lies within our feeble powers to study the awful teaching of the stars. The range of the puny instruments man can fashion is no measure, we may be well assured, of the universe as it is. The domain of telescopically visible space, compared with which the whole range of the visible universe of stars seems but a point, can be in turn but as a point compared with those infinite realms of star-strewn space which lie on every side of our universe, beyond the range,—millions of times farther than the extremest scope,—of the instruments by which man has extended the powers of visions given to him by the Almighty. The finite—for after all, infinite though it seems to us, the region of space through which we can extend our survey is but finite—can never bear any proportion to the infinite save that of infinite disproportion. All that we can see is as nothing compared with that which is; all we can know is as nothing: though our know-

ledge "grow from more to more," seemingly without limit. In fine, we may say (as our gradually widening vision shows us the nothingness of what we have seen, of what we see, of what we can ever see), not, as Laplace said, *The Known is Little*, but The Known is Nothing; not *The Unknown is Immense*, but The Unknown is Infinite.

THE BEETLE'S VIEW OF LIFE.

By Grant Allen.

HERE on a yellow crocus, buried deep in the beautiful golden cup, I have found a little bronze-mailed beetle, stealing the pollen as hard as he can eat it, and hugely enjoying his plentiful morning feast. I have picked him carefully out with a little bit of stick, and I have got him here now crawling about suspiciously upon my hand, and trying to find out what is the best way down from that unpleasantly warm and dangerously mobile valley—the hollow of my palm. I often wish I could discover how the world looks to that small creature here; and, perhaps, the question is not quite so unanswerable as it appears at first sight. When one remembers that brain and nervous system are on the whole a good index of mind, and that feelings and ideas depend upon the arrangement of the various sense-organs and their connected central parts, it will be clear that, after all, we may make a fair guess at what is passing in this little beetle's head, especially since his notions about things generally must in all probability be a good deal simpler and more directly dependent upon his sensations than our own.

Now, what, in the first place, are the beetle's senses? He can see, that we all know; and his sight is on the whole a good deal like our own. His eye can discriminate form, and that accurately, for in all flying creatures this sense is necessarily highly developed; it has been evolved and perfected side by side with their wings, or else they could never have learned to fly at all. They can doubtless distinguish colour, too; for we know positively from Sir John Lubbock's experiments that this is the case with bees, and there are good grounds for believing that the same thing is true of all flower-feeding insects as well, since all alike seem to be guided to the flowers by their brilliant hues. Sir John put drops of honey on slips of glass above bits of coloured paper; and when he had once taught a bee to feed from one slip, say the blue, he found that it would return straight to that slip, even when the relative places of the colours had been transposed. Now, almost all flowers which contain honey have also bright petals; and Mr. Darwin has shown that both honey and petals have been developed by the flowers for the sake of attracting insects, which carry their pollen from head to head, and so fertilise and impregnate the seeds. Moreover, the colours of the petals differ in different species, according to the kind of insects which they each wish to attract. Thus bee-flowers are usually blue or red; and Sir John Lubbock has proved that bees show a distinct preference for these colours, while beetle-flowers are often yellow, and small fly-flowers are generally white. Such facts, and others like them, show that the beetle has sensations of sight essentially identical with our own, and also that he has certain special tastes for certain special hues and blossoms.

It is much the same with the other senses. The beetle certainly hears sounds; and his hearing appears to be analogous to our own; for though he himself is not musical, yet many other insects are; and these produce special notes and melodies to charm the ladies of their kind. He can also taste, and is fond of sweet things, like most other animals, for the flowers which seek to allure him lay by a drop of honey for his use; and this liking for sugary juices is shared by almost all insects, from the flies which crowd around a barrel of treacle at a grocer's door, to the ants which suck the honey-dew from the little green aphides that they keep as we keep cows. Last of all, he can smell, for the flowers which depend on him for fertilization are usually perfumed, and both beetles and other insects are often attracted by scent, as all collectors well know; indeed, they frequently catch rare insects by enclosing one of their mates in a box, when the quick-scented and eager lovers soon sail up from leeward, evidently attracted by the distinguishing odour borne upon the breeze. Indeed, some butterflies have special scent-glands among the feathery scales on their wings, to make them more charming to their pretty spouses, just as so many of the higher animals have a peculiar musky perfume. I may mention that Mr. Darwin similarly sets down the brilliant colours and ornamental spots of butterflies, as well as the curious horns and excrescences of many beetles, to the long selective action of their fair lady-loves, who always choose the handsomest and strongest among their numerous rival suitors. It is to this same cause that we probably owe the bright iridescent hues and bossy headpiece of the little creature who has now just escaped from my hand by clumsily transferring himself to yonder tall blade of rank meadow-grass.

Thus, as far as his outward picture of the world goes, the beetle's ideas must really be very similar to our own. The universe of sights, sounds, smells, tastes, and touches through which he moves must present the same general effect as that which we ourselves experience in our intercourse with outer things. But when we come to consider the relations which the beetle establishes between these primordial sense-impressions, the little ideas and emotions which he elaborates out of them, we find signs that the difference is vast indeed. Though the material is the same, the product is as unlike as the letters of the alphabet are to the "Iliad" or "Paradise Lost." The elements of human thought are there, but the organising and co-ordinating power is wanting.

If you were to cut open the beetle's head, you would find in it a small knot or lump of nervous matter, roughly answering to our own brains. To this lump the various sense-organs send up bundles of nerves; and in it the impressions derived from the different senses are compared and arranged, so as to produce the common impulse upon which the beetle acts. But the size of this nervous knot is vastly smaller in proportion to the insect than the human brain is to the body of a man. Our brain consists of numberless cells, arranged and united in definite subordination to one another, and so disposed that every part of our nervous mechanism can be brought into relation with every other; while in many cases we are not concerned in our mental operations with actual sense-impressions at all, or even with memories of such impressions combined into the shape of ideal objects, but with wholly abstract conceptions, elaborated out of them by the action of the brain itself in its higher parts. The beetle, however, can do nothing analogous to this. Its mental life is wholly made up of direct impressions, and actions immediately dependent upon them. Memories it doubtless possesses in a slight degree, especially in the form of mere recognitions; but it is not probable that it can think of an object in its absence, or voluntarily recall it; while it certainly cannot reflect as we can about abstract ideas, or even about things which do not concern its immediately present needs and actions.

Indeed, the whole nervous system of the beetle is so loosely bound together—so little co-ordinated, as Mr.

Herbert Spencer puts it—that it can hardly be said to possess any distinct voluntary capacity, or any strongly-marked personality at all. In the case of man and the other higher animals, almost the whole nervous system is bound up with the brain, sending messages up to it, and receiving orders from it in return, so that a single great nervous centre governs all our movements, and ensures that uniformity of action without which the complicated activities of human life would be impossible. The only nerves (worth mentioning) in the human body which are not thus under the control of the brain, are those of the heart and other internal organs; and over these parts, as everybody knows, we have not any voluntary power. But all our limbs and muscles are moved in accordance with impulses sent down from the brain, so that, for example, when I have made up my mind to send a telegram to a friend, my legs take me duly to the telegraph office, my hand writes the proper message, and my tongue undertakes the necessary arrangements with the clerk. But in the insect's body there is no such regular subordination of all the parts composing the nervous system to a single central organ or head-office. The largest knot of nerve-matter, it is true, is generally to be found in the neighbourhood of the sense-organs, and it receives direct nerve-bundles from the eyes, antennæ, mouth, and other chief adjacent parts; but the wings and legs are moved by separate knots of nerve-cells, connected by a sort of spinal cord with the head, but capable of acting quite independently on their own account. Thus, if we cut off a wasp's head and stick it on a needle in front of some sugar and water, the mouth will greedily begin to eat the sweet syrup, apparently unconscious of the fact that it has lost its stomach, and that the food is quietly dropping out of the gullet at the other end as fast as it is swallowed. So, too, if we decapitate that queer Mediterranean insect, the Praying Mantis, the headless body will still stand catching flies with its outstretched arms, and fumbling about for its mouth when it has caught one, evidently much surprised to find that its head is unaccountably missing. In fact, whatever may be the case with man, the insect, at least, is really a conscious automaton. It sees or smells food, and it is at once impelled by its nervous constitution to eat it. It receives a sense-impression from the bright hue of a flower, and it is irresistibly attracted towards it, as the moth is to the candle. It has no power of deliberation, no ability even to move its own limbs in unaccustomed manners. Its whole life is governed for it by its fixed nervous constitution, and by the stimulations it receives from outside. And so, though the world probably appears much the same to the beetle as to us, the nature of its life is very different. It acts like a piece of clock-work mechanism, wound up to perform a certain number of fixed movements, and incapable of ever going beyond the narrow circle for which it is designed.

MR. MUYBRIDGE AND ROWING.

By the Editor.

THE method which has been applied so successfully to determine all the successive stages of a horse's motion in galloping at the rate of a mile in less than two minutes, can be much more easily applied to determine all the successive stages of an oarsman or sculler's action—for even the swiftest racing-boat does not travel faster than a mile in five minutes on still water, and by taking her against stream the problem would be rendered even easier. If rowing men of the Oxford, Cambridge, and the various Thames clubs would invite Mr. Muybridge to this congenial work, the principles of good rowing style and the secret of successful oarsmanship could very readily be determined. Hanlan might, I have no doubt, be persuaded to row past the twelve cameras, and so hand down to posterity the perfection of his marvellous style. Mr. Muybridge would be willing, I know, to do his part—his expenses being guaranteed, of course. I do not know what the expenses would be, but to judge from all that I have learned, they would lie somewhere between two and three hundred pounds, for a week's experiments, with all necessary assistance in arranging for the sets of twelve instantaneous pictures. He would willingly supply contributors with copies (free of further expense) for study and comment. The value of such views at the present time, and their interest hereafter, when, perhaps, new methods of rowing may have come into vogue, and when, at any rate, oarsmen will like well to know how their predecessors rowed, can hardly be over-estimated. If the presidents of Oxford, Cambridge, London, and other clubs, care to see anything done in this direction, arrangements could be quickly made (at present Mr. Muybridge's apparatus is at New York, but it could be here in a fortnight), and the expenses readily subscribed. I should be very willing, for my own part, to put my name down for £50, if that sum would bring the amount up to the required total. If, however, there were good prospect of the amount being readily made up otherwise, I would content myself with such a subscription (five or ten guineas) as hundreds of boating men would, I am sure, be glad to offer for so invaluable a contribution to the scientific investigation of oarsmanship.

American Agriculture *v.* English.—Mr. Fowler, M.P., for Cambridge, recently gave to a Chicago *Tribune* reporter his impressions of the agricultural resources of the West, where he has spent some time in making observations. "What has interested me most," said he, "is the matter of transportation to England, in connection with the cost of production there, and the question is whether we can continue much longer to compete with America in the raising of wheat, or even to raise it at all and make it pay. The natural protection to English production, by reason of the cost of carriage, must be—nay, is—rapidly diminishing, and I rather expect, if we were to have a good harvest in Europe and America at the same time, you would have prices such as we have never expected." "The American farmer is producing and transporting wheat and corn so cheaply, then, that his English brother cannot compete with him?" "It is a good deal as a gentleman expressed it to me the other day, when he said: 'A man out here in Iowa is competing with the English farmer just as if he lived in Yorkshire.' That may be a strong way of putting it, but you must observe the great advantages which the American farmer has over the farmer on the other side. Iowa land, for instance, costs $10 an acre, while in England it costs £50, £70, or £80 an acre, so that the Englishman is terribly handicapped at the start, for he has to pay interest on £50 to £70, while the Iowa man pays interest only on £2. Then, in addition to all that, the Iowa man has a better soil and a better climate. In short, with these advantages in favour of the American farmer, with the cost of transportation minimized as it is, so that our natural protection from that cause is rapidly diminishing, I have great doubts whether the cultivation of wheat will pay in England at all. I speak not so much of the present as of the future, for our crop this year has been a good one, while yours seems to have been just the other way. Your deficiency this year, as I have seen it stated, is 80,000,000 bushels—nearly as large as England's entire production in an ordinary season. But here is your vast expanse of territory developing every year. Then, again, you virtually raise wheat in this country by machinery. The extent of your wheat-raising territory is simply astounding, but your population, while large in the aggregate, is spread over these vast expanses, and your real market is elsewhere—across the water, over in England, where we find a contrary state of affairs: a comparatively small wheat-raising area, with millions of people to be fed. And I don't begrudge you your good fortune in the least. Your prosperity is ours, for, unless our people be cheaply fed, they cannot afford to work for reasonable wages, and unless we can manufacture at reasonable cost, we can no longer hope to supply the world with our manufactured products."

XUM

COLLISIONS AT SEA.

By the Editor.

IT has been no unusual circumstance for two ships in open daylight, and in calm, clear weather, to be brought into collision through what landsmen might regard at first sight as absurd blundering, though seamen know that the indications which should have guided the manœuvres of either ship have been quite naturally misunderstood. In point of fact, the risks of collision at sea may be divided at present into two broad classes—the avoidable risks, and those which are unavoidable. Of the latter class of collisions it would be useless to speak; but of the former there is much to be said, and for their prevention much might well be done.

The first and most striking circumstance in the history of all such collisions as might in reality have been avoided, is the utter absence of any proper means of communication. Flag signalling is, of course, very often a ready and convenient method; but under certain conditions, of not unfrequent occurrence, it fails either wholly or in part. The wind may be insufficient to display the small signal flags, or may be so light and variable that they are not quickly or readily discerned. Again, the wind may blow in such a direction that the flags, though well displayed to viewers in other directions, are invisible to those for whom they are intended. Then anyone who examines the flags used in signalling will note that, although when the whole of each flag is squarely shown, one cannot possibly be mistaken for another, this is by no means the case when the flags are exhibited at some distance, in light winds, and under varying atmospheric conditions.

I have before me as I write pictures of a set of signals devised by Mr. A. Stewart Harrison, in which these objections are entirely obviated. In the first place, all the signals are of the same kind, and formed in a similar manner. In each signal there are two vertical rows of bright discs on a black ground, which may be set facing in any direction. (The discs at night can be illuminated either by reflected or transmitted light, as may be most convenient.) Each row may show any number of discs from 0 to 5, and as the eye can in a moment tell whether one, two, three, four, or five balls are exhibited in each row, any combination is at once recognised, and can be immediately replaced by the next, and the next, and so on, till the necessary message has been spelled out. Nothing could possibly be simpler or less likely to be misunderstood than this method of signalling. The discs could be discerned with the naked eye at a considerable distance in ordinary weather; and could be read off at two or three miles distance, in clear weather, with a telescope. The actual distance at which two discs of given dimensions could be separately discerned with a telescope is known already to astronomers from the experiments which have been made by Dawes, O. Struve, and others on artificial double stars; and it can readily be shown that with such dimensions as Mr. Harrison proposes for the discs, the range of distance above mentioned would be well within the powers of this method of signalling. It is not too much to say that if this method were adopted, a large proportion of the collisions which now occur would be rendered impossible, or possible only through utter negligence on the part of all concerned in directing the course of the two ships.

It is singular, however, that even those whose safety depends on the use of such methods seem unready to adopt any improvement in signalling at sea. After a method has been shown by experience to be quite inadequate, reliance continues to be placed upon it as though it had never failed. As an instance in point, consider the ships lights now in use. I remember, fourteen years or so ago, writing an article for the *Daily News* on a lamentable collision which had then recently occurred in consequence of the want of any means of recognising, in good time, a change in the course of one or other of two approaching ships. Nothing could, in a scientific sense, be less suited to the requirements of the case than the actual arrangement of a ship's lights. That a light on the port side should be of one colour, and a light on the starboard side of another, is well so far as it goes. But it does not tell another ship much. The approaching ship might change her course considerably, and yet show the same two colours, not greatly changed in apparent position. If only two lights are to be shown, these might be much more effectively placed than as at present. Suppose, for instance, a red light were carried in some well-chosen position amidships, and a green light near the bows, and at a lower level, both being so placed that they could be seen well from either the port or starboard side, or in front. Then an observer on an approaching ship would know on what course a ship carrying these lights was steering. If the green light were seen to the right, he would know that he was on her starboard beam; if to the left, that he was on her port beam. If the red light were directly above the green, he would know that she was bearing full upon him. If she changed her course, the two lights would be brought either nearer together or further apart. At present there is absolutely no indication of another ship's position (on a dark night when only her lights can be seen), unless she is steering on such a course that no precautions are required to avoid her. That is to say, if a ship, still at a moderate distance, is sailing in a course which will not bring her at all near another, an observer on this other ship will know what that course is, for he will see either a red light only or a green light only, according as her course is (in non-nautical terms) from right to left or from left to right. But if she is bearing nearly towards his ship, he will see both her lights, and nearly in the same relative position, whether her course would carry her past his ship (if it were at rest) on one side or on the other side. He has no means of knowing, therefore, to which side he should direct his course. There are the rules of the road at sea, of course, and if these were always, or could always be, strictly followed, the present arrangement of a ship's lights would serve well enough. But it is only necessary to read the reports of cases in our Admiralty Courts to learn that instances frequently occur where the rules of the road cannot be—or, at any rate, are not—followed. Even in broad daylight, and in clear weather, collisions have occurred when the vessels have been cognisant of each other's actual course and changes of course for a distance of two miles before collision actually took place, the observed manœuvres having been simply misinterpreted. How largely must the risk of collision be increased at night, in hazy weather, or under conditions otherwise unfavourable? Yet nearly all that is wanting to prevent such collisions absolutely, and all that is wanting to render them infrequent, is an arrangement by which not only the course, and any change of course, of each of two approaching ships may be quickly made known to the other, but also (as in Mr. Stewart Harrison's ingenious arrangement) by which each ship may quickly convey directions or warnings to the other. It is the interest of all who travel by sea, and the duty of all who care for the safety of our seamen, to urge that the simple measures required for preventing avoidable collisions should as soon as possible be carried out.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

BEFORE beginning our examination of the Constellation Virgo to-night, we will return to that of Hydra for the purpose of looking at a very wonderful object, omitted in our description on p. 376. The student will find it by fishing with a power of 100 or so about 2° (four diameters of the moon) to the south of μ Hydræ (map p. 474). It is No. 27 of Herschel's IVth Catalogue, and is one of the most remarkable planetary nebulæ in the heavens. Unlike nebulæ generally, this will bear considerable magnifying power. It will be seen as a pale-blue disc, looking just like the ghost of Jupiter. As Huggins has shown that it is gaseous, the sharpness of its outline is very curious.

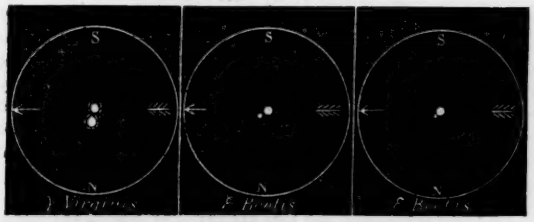

Fig. 26. Fig. *a*. Fig. *b*.

Turning now to Virgo, we will begin with that most interesting star γ, which is shown in Fig. 26, as seen with a power of 160. When first observed by Herschel, in 1790, the components of this star were nearly 6″ apart, but were approaching each other; and in 1836 were so practically superposed as to appear single under the very highest power that Admiral Smyth could apply to them upon his 5·9 inch achromatic. Since that time they have been separating, and their distance at present amounts to about 5″, so that they form an easy pair in the instrument we are using. θ Virginis (map, p. 474) is a very pretty and interesting triple; the third star, which is nine times as far from the large one as its more obvious companion, will require a dark night and pretty sharp sight to see it well. There are very many beautiful and interesting pairs of stars in Virgo; but as they are mostly below the sixth magnitude they are not marked in the monthly maps in Knowledge, and no amount of description would enable the reader to identify them. Fortunately, simple sweeping in the marvellous region to which we are about to introduce the reader, will suffice to enable him to pick up many of the wonderful mass of nebulæ collected within the area roughly bounded by ϵ, δ, γ, η, and β Virginis, and β Leonis. If the student will arm his instrument with a power of about 40, and sweep slowly over that part of the sky contained within the curve drawn through the stars we have named (map p. 474), he cannot fail to be astonished and pleased at the wealth of nebulous objects, and the pretty fields of stars that he will encounter. One of these curious objects is shaped like a boy's kite. A few are resolvable into stars in some of the enormous telescopes now comparatively common. Others are unmistakably gaseous.

Above Virgo is situated Coma Berenices, easily recognisable in the sky by the coarse cluster of stars in its north-western portion. If the reader will draw an imaginary line from α through 36 on the map (p. 474), then at about three times as far to the right of 36 as 36 is to the right of α, and a little above such line, will be found 24 Comæ, a wide double star, but interesting from the beautiful contrast of orange and pale-purple presented by its components. Just above, and to the left of α Comæ (map p. 474), what will appear like a nebula will be found. It is 53 of Messier's Catalogue, and is really an immense mass of tiny stars; but it requires a much more powerful instrument than ours to show this. Other nebulæ, mostly faint, will be found among the cluster of stars to which we have previously referred.

Adjoining Coma Berenices above lies Canes Venatici, of which the chief star α, 12, or Cor Caroli—for it has all three designations—is a widish double. About one-third of the way between Cor Caroli and δ Leonis 2 Canum will be found; a close pair, with prettily contrasted colours. There are numerous other pairs in this constellation, but, for the so often reiterated reason, we can give no intelligible directions for finding them. In the case of more than one of the remarkable nebulæ, however, contained in it, we trust to be more successful in pointing out their whereabouts. 3° (6 diameters of the moon) to the south-west of γ Ursæ Majoris, the star at the end of the Great Bear's tail, will be found two rather dim nebulæ, nearly touching each other. This is Messier 51, the astonishing Spiral nebula, which, as seen in Lord Rosse's great telescope, has been pictured in so many works on astronomy. About midway between Arcturus and Cor Caroli, but rather nearer the former (map p. 474), will be found a bright nebula, Messier 3, which large telescopes resolve into a brilliant condensed cluster of minute stars. Some $2\frac{1}{2}$° to the north-west of Cor Caroli, is a nebula, 94 Messier, which, though small, is sufficiently conspicuous in the class of instrument we are using. Other nebulæ in this constellation may be picked up by fishing, especially in the region between α Canum Venaticorum and ξ Ursæ Majoris.

[As but one figure came from our esteemed contributor, F.R.A.S., this week, we have ventured to add two doubles, viz., ξ Boötis, fig. *a*, and ϵ Boötis, fig. *b*. Both stars are shown in our monthly map, No. 22, for March 31.—Ed.]

THE AMATEUR ELECTRICIAN.

ELECTRICAL GENERATORS.

THE task we have set ourselves in this series of papers is not by any means an easy one. Our object is to help, by practical and easily-followed-out instructions, those who desire to become experimenters in this most interesting department of physical science. There is no "Royal road" to a thorough knowledge of the laws of electricity, any more than of any other science, and our readers must, therefore, please understand that while every effort will be made to render each branch of the subject dealt with as complete as possible, a "text-book" on electricity is beyond our scope. It is anticipated that we shall soon be at liberty to initiate a series of papers on the rudimentary demonstrations of electrical properties, but here the assumption must be allowed, that our readers know just a little of the science.

The present production of large quantities of electricity—larger than were ever conceived to be possible—necessitates some agency other than the galvanic battery. Indeed, the battery could never have yielded so bountiful a supply of electric force as is now being utilised in very many places—notably the Crystal Palace Electrical Exhibition. Why this is we will not attempt to explain now, although it may be found advisable to do so hereafter. It is not difficult to conceive that one of the features in the

galvanic battery is the transference or *motion* of particles of matter from one state of combination to another. Thus, in the Daniell cell, the sulphate of copper is converted into sulphuric acid by the means of hydrogen, which displaces the copper, the sulphuric acid being likewise converted into sulphate of zinc by particles or atoms of zinc taking the place of the hydrogen.

There are, however, other forms of motion which in their turn are capable of generating currents of electricity, motion not necessarily of minute particles, but of large masses of matter. If one of two masses moves or passes before the other, more particularly if magnetism or electricity is present in one of them, an electric current is generated. Let us imagine, in the first place, that we have a hollow helix or coil of copper wire, such as that contained on the leg of an electro-magnet, or such as we should get by carefully winding an insulated copper wire round a thin stick of wood. Now, suppose the two ends of the wire are connected to a galvanometer. Then on plunging a steel magnet into the coil, the needle of the galvanometer will be deflected, say, to the right, and will indicate the passage of an electric current. When the magnet is withdrawn, the needle will be again deflected, this time, however, to the other side, thereby indicating the passage of a current in the opposite direction to the previous one. Let us pursue this a step further. Our readers are doubtless aware that when a piece of soft iron is placed in the vicinity of a magnet, magnetism is immediately induced in it. If, then, we place a core of soft iron in our coil of wire, and bring a magnet near it, it becomes at once magnetised. The core being already inside the coil, magnetising it in that position is equivalent to plunging in a magnet *instantaneously*. A current of electricity is consequently produced, but, of course, of greater strength than when the magnet is plunged into the coil. Similarly, on removing the inducing magnet, the iron core becomes at once *demagnetised*. This is identical to withdrawing the magnet, and therefore a strong current is generated or induced, travelling in the *opposite direction* to the preceding one.

What have we done? We *moved* a magnet, and obtained a current of electricity. To a certain degree, the current produced is a measure of the force expended in moving the magnet, and it is interesting to notice that when the coil circuit is complete, it is more difficult to move the magnet than when the circuit is broken. This phenomenon will, however, be referred to more fully on a future occasion. The experiments above described illustrate the principles of what is known as the magneto-electric machine, such as those used for medical purposes, in which a coil of wire and its core of soft iron are made to approach and recede from a permanent magnet, which is fixed. We shall, in our next, give instructions for making a small machine, not one of those used by the medical faculty, but one of a much-improved form, which will be capable of doing all that a galvanic battery will do.

The apparatus for performing the experiments described in this paper may be very cheaply constructed, and our readers are strongly recommended to try them, in order that they may better understand our subsequent papers on the subject. The coil may be made by winding five or six layers of No. 22 cotton-covered wire* on a paper tube, about half-an-inch in diameter, and three inches long. After the wire is wound on the tube, it would be as well to coat it with a little shellac varnish, made by dissolving shellac in methylated spirit. The magnet may be of the ordinary round form, such as are used in telephones, and should fit the tube easily. A piece of soft iron of similar dimensions will complete this portion of the apparatus. The galvanometer may also be easily made. Magnetise an ordinary sewing-needle by rubbing it a few times in one direction over one pole of the steel magnet, and then suspend it by a thin thread (or, better still, by a fibre of unspun silk), in an oblong coil, consisting of about ten turns of the No. 22 wire, just long enough to allow the needle to rotate freely.

* Messrs. Rickard, of Derby, are wire makers of the highest repute in electrical circles. Their price for No. 22 is 1s. 10½d. per pound.

THE THREE COLD DAYS OF APRIL.

By the Editor.

FEW weather phenomena in this country are more remarkable, and seem at present less easily explained, than the so-called "borrowing days," as they are called, between the 10th and 14th of April, when usually the temperature falls considerably below that due to the time of year. The cold at this time is, at any rate, sufficiently marked—first, to have attracted long since general attention; and, secondly, to affect in a very obvious manner the average temperature for these days during the last eighty years. We find these three cold days of April, which before the change of style were the first three days of the month, thus described in doggrel lines in the north of England:—

"March borrows from April
Three days, and they are ill;
The first of them is wan and weet,
The second it is snaw and sleet,
The third of them is a peel-a-bane,
And freezes the wee bird's neb tae stane."

The following lines are given in the "Glossary of Scotch Words and Phrases":—

"Said March to April,
Gie me three hoggs upon yon hill;
And in the space of days three
I'll find a way to gar them die.
The first a bitter blast did blaw,
The second it was sleet and snaw,
The third it cam sae full a freeze
The birds' nebs they stack to the trees;
But when the days were past and gane
The three puir hoggs cam hirplin hame."

This is manifestly an imperfect version of the lines in the poem called the "Complaynt of Scotland," where the reference to the borrowing of three April days is much clearer (in the above account March borrows hogs not days):—

March said to Aperill
I see three hogs upon a hill;
But lend your first three days to me,
And I'll be bound to gar them dee.
The first it shall be wind and weet,
The next it shall be snaw and sleet,
The third it shall be sic a freeze,
Shall gar the birds stick to the trees.
But when the borrowed days were gane,
The three silly hogs cam hirplin hame.

This is, I believe, the oldest version of the doggrel. It belongs to a time when the three cold days of April really were the first three days of April. The other was perhaps modified to correspond with the new style, according to which the cold days fall in the heart of the month, and cannot be very well imagined to be borrowed by March. It is worthy of observation how correctly common observation has indicated the true position of these cold days; for in the temperature curve derived from three quarters of a century of accurate observation at Greenwich, the depression corresponds exactly with the days which before the change of style were the 1st, 2nd, and 3rd of April.

THE ECLIPSE OF MAY 17.

WE propose to give next week a small map showing the course of the moon's shadow during this eclipse. The editor has appointed Mr. Richard A. Proctor his special correspondent in Egypt on this occasion; but whether Mr. Proctor's numerous engagements will allow him to accept the appointment is not as yet known.

THE NEW MOON IN APRIL.

THIS is the way in which our usually calm and reflective contemporary, the *Scientific American*, tells us that the moon will be in conjunction with four planets on the 18th and 19th inst. :—

The new moon of the 17th commences her course with a brilliant record. On the 18th, the day after her change, she pays her respects to three planets—Venus, Saturn, and Neptune—on the same evening. It is difficult to see the moon when a day old, for the crescent is but a slender thread, still it can be done. If the evening be exceptionally clear, the keen-eyed observer may behold the lovely picture, the moon passing about two degrees north of Venus and three degrees and a half north of Saturn. But the loveliest exhibition of the month will occur on the 19th, when the two days' old crescent will be in conjunction with Jupiter, and only forty minutes north of him. As the moon does not set until after 9 o'clock, there will be ample opportunity for seeing the show, if the clouds are kind.

VENUS IN APRIL, 1882.

THE *Scientific American* has, indeed, suddenly become quite poetic over the planets. Here, for instance, is its account of Venus in April, 1882 :—"Venus is evening star, and the only one among the planets whose movements excite a marked interest during the month. She has now advanced far enough in her eastern course to be seen in the west soon after sunset, and to allow the observer to obtain a glimpse of the beauty to be revealed during her nearly ten months' course as evening star. She will soon be the brightest in radiance, the largest in size, the softest in colour, of the myriad golden points that glitter in the celestial archway. Neither is she to be considered alone in an æsthetic light. The Queen of the Stars has unwittingly a mission to perform, when, closing her career as evening star in December with the grand event of the transit, she furnishes the inhabitants of the planet that shines so brightly in her sky, one means for measuring the unapproachable, the much-desired solution of the problem—the earth's distance from the sun."

THE NEW COMET.

THIS comet, called "Comet Wells," is now passing from near Vega (Alpha Lyræ) into Cepheus, where its course will change, in May. We propose to give next week a map showing a portion of its path. The following reaches us from America (*Scientific American*) :—

The elements of the orbit of the new comet are: Perihelion passage, June 15; perihelion place, 49 deg. 35 min.; longitude of node, 206 deg. 40 min.; inclination, 74 deg. 47 min.; perihelion distance, 10,000,000 miles; motion direct.

This comet appears to have no analogue in the past, as no comet is known with elements sufficiently resembling these to constitute reasonable belief in identity. The elements of the comet of 1097 somewhat resemble those of the present comet, but the perihelion distance of the former is computed to be seven times as great as that of the latter.

At present the comet is about 160,000,000 miles from the earth, and its distance from us will probably not be less than 80,000,000 at any time, though further calculations will be necessary to settle that point. It may be expected to make a fine display for a few days in the early part of June. The present extraordinary intensity of its light, which comes to us from the enormous distance of 160,000,000 miles, proves that it has plenty of material for future display, and it will probably show a long and nearly straight tail of enormous dimensions to our antipodes. How much it will give us is still problematical.

Professor Maspero is said to have succeeded in making satisfactory terms with the villagers whose dwellings and mosque encumber the temple of Luxor, his only difficulty being with Mustapha Aga, the local British Consul, whose demands are considered exorbitant. The temple is likely to yield results of the highest archæological interest. It was begun by Amenhotep III., carried on by Seti I., Rameses II., Horus Sabaco, and Alexander Ægus; and the great pylons erected by Rameses II. are sculptured with battle scenes similar to those at Aboo-Simbel, and inscribed with a version of the heroic poem of Pentaur.

The Formation of Coal.—All attempts to explain satisfactorily the formation of coal have thus far proved unsuccessful, though it is generally understood that it is the product of the decomposition of vegetable matter. Just how that decomposition has been brought about chemically is a matter which chemists have not as yet been able to solve. The principal difficulty has been that it has been impossible to obtain a clear insight into the chemical constitution of coal. It has been thought hitherto, and this is still the popular belief, that coal is in the main pure carbon, mixed with varying quantities of bituminous substances. It has been generally believed that, as the product of the distillation of coal is principally carbon, it would be safe to conclude that free carbon actually does exist in coal. The fact that sugar, starch, &c., under similar circumstances leaves a residuum consisting of carbon has never been considered a proof that that element existed in these bodies in a free state. It is well-known that coals which may have the same percentage of carbon, hydrogen, and oxygen, do not by any means, in coking, yield the same products of distillation, and we have a complete analogy for this in the behaviour of cellulose and starch when subjected to distillation. Evidence points to the conclusion that coal is a mixture of many and complex compounds; and the difficulty, amounting almost to an impossibility, of separating these compounds has much to do in rendering a chemical solution of the questions involved in the formation of coal a very arduous task. The production of coal by artificial means is met by great obstacles, among which the absence of all knowledge concerning the conditions under which that process actually took place is the principal one. The question whether the vegetable matter to which our coal veins owe their origin was amassed by drifting, or was carbonised *in situ*, has been much debated, and there has been much discussion on the point whether it was obtained from water or from land plants. Dr. Muck, of Bochum, in a recent work to which we shall refer at greater length in the future, takes up the theory that algæ have mainly contributed to the formation of coal. It is urged that the remains of marine plants are rarely found in coal veins, and that shells, &c., are not often met with. Dr. Muck calls attention to the fact that marine plants decompose easily and completely, losing their form entirely; and that the disappearance of the calcareous remains of mollusks is readily explained by the formation of large quantities of carbonic acid gas during the process of carbonisation. In accepting the marine origin of coal, it is not necessary to resort to the assumption of immense pressure and high temperatures to explain decomposition and the total destruction of the structure of the original substance. Dr. Muck combats Fremy's bog theory at length. His views are well supported by recent investigations made by Herr P. F. Reinsch, who has examined 1,200 sections of coal, coming to the conclusion that that mineral substance has not been formed by the alteration of accumulated land-plants. Herr Reinsch claims to have discovered that coal consists of microscopical organic forms of a lower order of protoplasm; and though he carefully examined the cells and other remains of plants of a higher order, he computed that they have contributed only a fraction of the matter of the coal-veins, however numerous they may be in some instances. —*Scientific American.*

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RAPID MOTIONS PHOTOGRAPHED.

To a people like the English, interested by inherited tendencies in all active exercises, and especially in horsemanship, the experiments made by Mr. Muybridge on the movements of galloping horses, running dogs, &c., ought to be full of interest, and his successful mastery of most difficult problems deserves their highest recognition. A few years ago the news that a photographer of San Francisco had succeeded in taking photographs of a galloping horse was received here with incredulity. Now we have the photographer himself among us, and the means of studying his pictures in such sort as to remove all possibility of doubt as to the reality of his triumphs over difficulties which had naturally enough been regarded as insuperable. That a horse rushing along at the rate of a mile in 1⅘ minutes, and moving its limbs forward in part of each stride with nearly twice that velocity, should be seized by photographic art so as to show every limb well and clearly delineated, would have seemed wonderful indeed to the earlier professors of that art. Still more amazing is it to find ten or twelve distinct pictures taken during a single stride, the comparison of which *inter se* enables the most rapid of all equine movements to be analysed as thoroughly as though the horse could be made to go through all the movements of the swiftest gallop at a funereal pace. Then, by combining these together in a much improved kind of zoetrope—the zoopraxiscope—the horse can be made to go through the action of galloping as perfectly as though he were actually galloping before the eyes of the observer. This Mr. Muybridge has done for the galloping and leaping horse, the trotting and walking horse, the dog running, leaping, and in the chase, cattle, wild bull, deer, athletes, gymnasts, and even for birds. Not only the characteristic movements of the different actions, but even those slight and scarce definable peculiarities which distinguish the movements of one athlete from another in performing the same feat, of one horse from another in moving at the same gait, and so forth, are perfectly recognisable in the combination of pictures which, separately seen, simply startle us by the new light which they throw on the real nature of these rapid motions.

Whether figures thus unfamiliar should replace the conventional forms by which artists have tried to represent swiftly moving horses, men, and other animals, is a point about which we apprehend that art and Mr. Muybridge may be some time at issue. But that the utterly incorrect conventional pictures should go by the board (whatever compromise should replace them) there can be very little doubt. It is well known to artists that wrong colours and incorrect shades have to be used to produce particular effects; and it may well be that positions never at any single moment assumed by an animal, may better suggest the idea of motion than the somewhat ungainly positions which are (*not assumed, be it noticed, but are*) passed through by swiftly-running animals. But it is quite certain that pictures so utterly and unnecessarily wrong as those which have hitherto done duty (save in a few exceptional cases) for moving animals, must be improved out of existence.

ELEPHANTS.

Before the Jumbo mania has quite passed away, it may be well to place before the readers of Knowledge a few facts in connection with the distribution of elephants, past and present.

It is well known that the two existing species of elephant are found, one in Southern Asia, the other in Africa, and this represents their distribution ever since the dawn of history; but probably few of the multitude lately thronging the Zoological Gardens to see the gigantic African specimen lately shipped to America, knew, or cared to remember, that *perhaps* under their feet reposed the fossil remains of the much more gigantic ancestors of the great beast they came to admire, which once roamed wild through British forests; and that in the land to which he has been consigned, now quite destitute of wild elephants, abundant traces are found of animals of the same family long since extinct. The greater part of those found fossil in America belong to the gigantic Mastodon, which differed from the elephant in the form of its teeth, and in other peculiarities, although resembling it in general form. "The bones and teeth of the Mastodon," says Mantell, "have been found throughout the plains of North America, from north of Lake Erie, to as far south as Charleston in South Carolina. There were also Mastodons peculiar to Central and Southern America. The remains of other species have been discovered in the Crag of England, in France, Switzerland, Germany, Spain, and Italy, in Asia Minor, and in several parts of India." But in addition to the remains of Mastodon, both America and Europe can show geological records of a nearer relative of the elephant, in the fossil remains of the Great Mammoth, which are so abundant in Siberia, and which are found also in India. There is more than one species of elephant found fossil in England, but the Mammoth is the chief, and great interest attaches to it, because although doubtless it has been extinct for an immense period, its remains are found both in England and France, associated with the rough stone (Palæolithic) implements of early man. In the river gravels and caves of England, many fossil elephants have been found with the weapons and tools of the wild hunters by whom probably they had been slain; and all doubt as to the co-existence of man with this huge extinct animal is set at rest by the discovery, in one of the French caves, of a drawing of the Mammoth, made by the cave dwellers on a part of a tusk of the animal. A cast of this remarkable drawing may be seen in the British Museum, and by it we see that this extinct elephant was covered with long hair and wool, and had enormous tusks; the truthfulness of the representation having been proved by the discovery in Siberia, in 1799, of an entire carcase imbedded in ice, where it had lain for unknown ages.

The great interest of these discoveries lies in the fact of their being found in the cold regions of Northern Europe, Asia, and America, whilst at present elephants do not range farther north than 30° N. The woolly covering of the Mammoth doubtless enabled it to endure great cold, but it is certain that at present it could not find sustenance in those regions where it was formerly so abundant; therefore its presence denotes that a great change of climate has taken place in those regions, a fact confirmed by many other geological observations, proving that a warm temperate climate and abundant vegetation once existed within a few degrees of the North Pole.

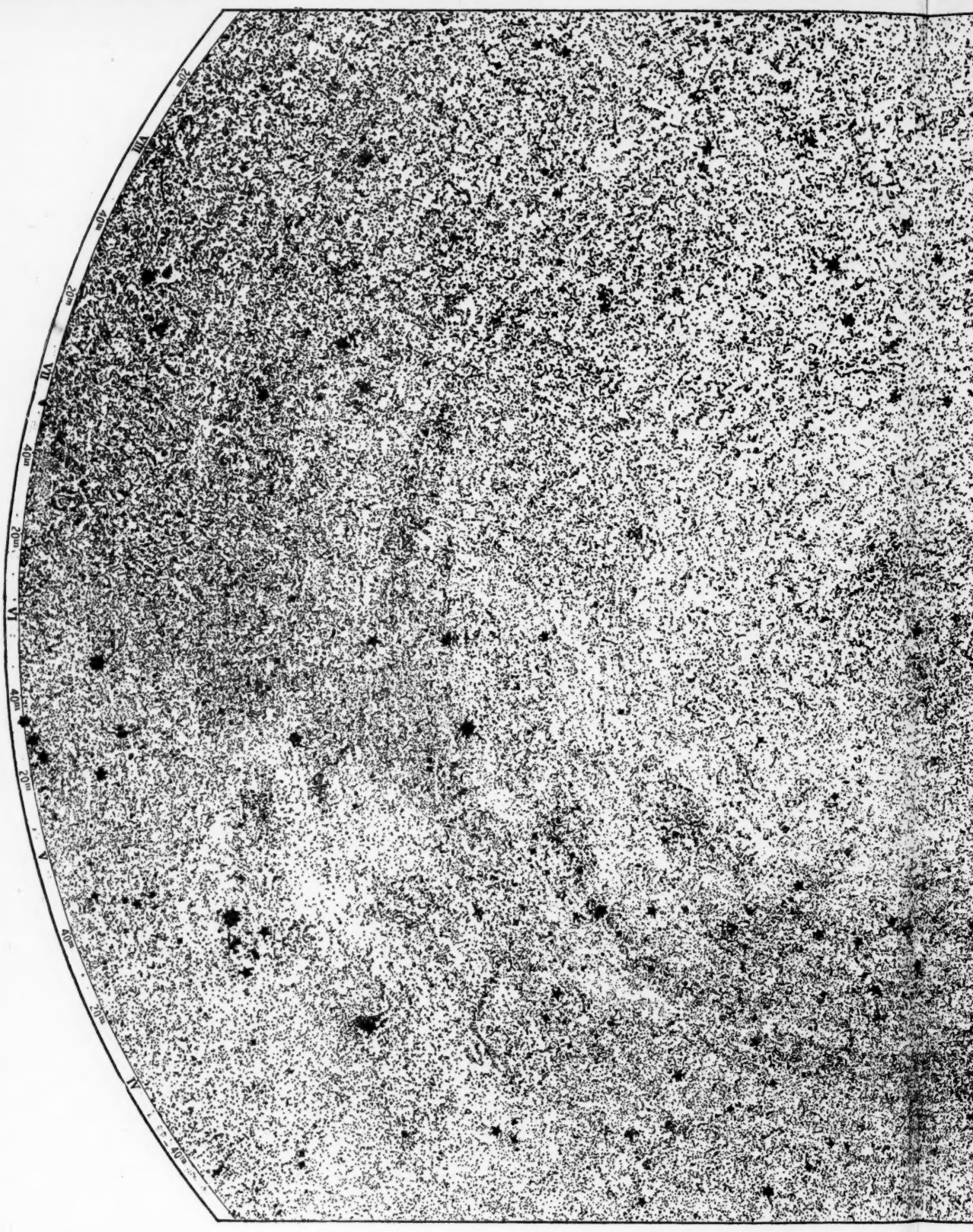
The question then arises as to the birth-place of these gigantic ancestors of the elephants of Asia and Africa. From the abundance of the fossil remains in the countries before-named, it seems probable that they originated in the north; nevertheless, it must be remembered that the bones of many species of elephant have been found among tertiary deposits both in India and Europe, and how they managed to spread themselves over the American continent and into Africa is a problem of general, as well as scientific interest. It is evident that they do not owe their distribution to human agency, for although man has been proved to have co-existed with the Mammoth, it is certain he did not then possess cranes and steam ships to convey huge beasts across the ocean; it therefore follows that at the time when Mammoths ranged freely over Britain, our present island was not divided from the continent by the waters of the Channel; this shows a considerable change in the distribution of land and water, but the change must have been still greater in the North, to enable the Mastodon and the Mammoth to pass between America and Asia. As to Africa, which does not appear to have been the original home of the elephant, it was undoubtedly at one time united to Europe by land in the Mediterranean, which allowed the passage, not only of elephants, but of other great beasts now exclusively African, as the rhinoceros and hippopotamus, which are also found fossil in England; but this connection had been broken long enough to allow of the total extinction of the Mammoth in Europe, and the rise of the new species of elephant to which Jumbo belongs, in Africa, long before the dawn of history.

This subject has been ably treated by Mr. Wallace in his books on the "Geographical Distribution of Animals," and "Island Life," to which I would refer such readers of Knowledge as may desire more information on these most interesting questions; meanwhile, it is possible that the deportation of Jumbo to America may in the distant future restore the elephant to that great continent. We know that ancestors of the horse are to be found fossil in America, but the horse was unknown to man on that continent before the Spanish conquest, though now present in vast wild herds, so likewise it is possible that Jumbo's descendants may at some time range over the prairies as the Mastodon did of old; and naturalists of future centuries may, perhaps, trace to the favourite of the British Zoological Gardens the rise of a new species of elephant in America.

A. W. Buckland.

[346]—Accumulative Sinking Fund.—The following table, showing the number of yearly drawings, (each increasing by the interest on the bonds redeemed) required to pay off loan at par) will be found useful:—

Int. on Loan.	¼ p. c.	1 p. c.	1½ p. c.	2 p. c.	2½ p. c.	3 p. c.	5 p. c.
3	65·83	46·90	37·17	31·00	26·67	23·45	15·90
4	56·02	41·04	33·13	28·01	24·36	21·60	14·99
4½	52·31	38·73	31·49	26·78	23·39	20·82	14·58
5	49·15	36·72	30·05	25·68	22·52	20·10	14·21
6	44·02	33·39	27·62	23·79	21·00	18·85	13·53
7	40·03	30·73	25·64	22·23	19·73	17·79	12·94
8	36·81	28·55	23·98	20·91	18·65	16·88	12·42
9	34·17	26·72	22·58	19·78	17·71	16·09	11·95
10	31·94	25·16	21·37	18·80	16·89	15·38	11·53



Section of an equal-surface Chart of 324,198 Stars, each point representing a

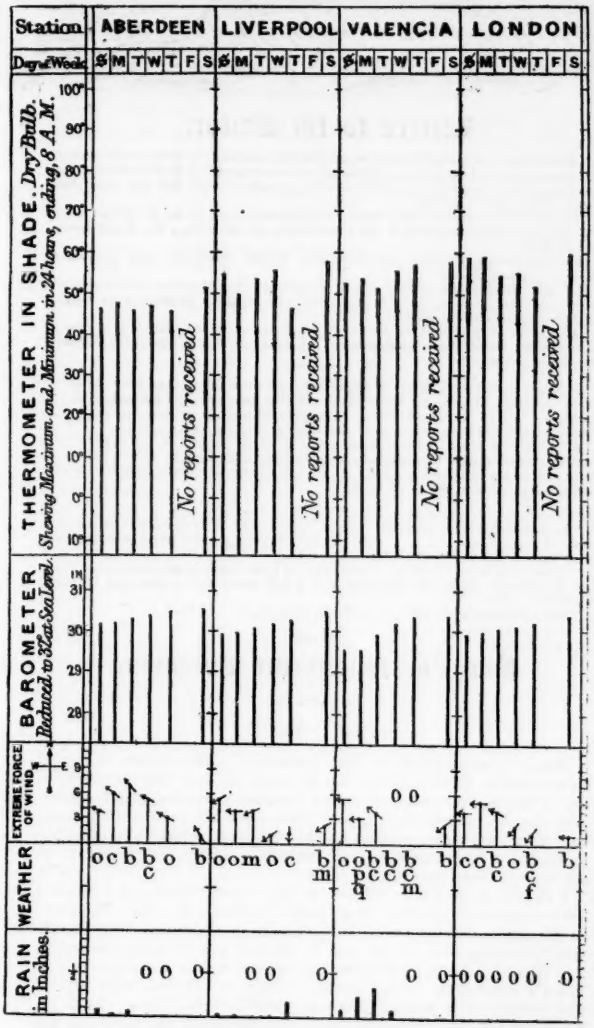
representing a Star in Argelander's Great Catalogue.—R. A. Proctor, *del.*

XUM

Sta		THERMOMETER IN SHADE. Dry Bulb.	BAROMETER	EXTREME FORCE OF WIND.	WEATHER	RAIN
D	H					

WEATHER DIAGRAM.

FOR WEEK ENDING SATURDAY, APRIL 8.



WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

CANALS ON THE PLANET MARS.

IN a letter to the *Times*, Mr. Webb says:—It has long been known that the surface of the planet Mars is so mapped out into brighter and darker portions as to suggest the idea of continents and oceans, and the analogy thus implied with the arrangements of our own globe is strengthened by the existence of brilliant white patches, as of snow or ice, situate at or near the planet's poles of rotation, and varying in extent with its changing seasons, as well as by occasional differences in outline or colouring, which may well be explained by the supposition of a vaporous atmosphere.

In the autumn of 1877 and spring of 1878, when the planet was in a part of its orbit which presented its surface advantageously to view, a number of minute, straight, black or dusky bands were detected by Schiaparelli, traversing and subdividing the supposed continents in various directions. These have been called from their aspect "canals," though, of course, their scale entitles them rather to the appellation of straits, or very long, narrow arms of the sea. A few of these had been previously seen by various observers, but to the Italian astronomer belonged the credit of developing and delineating them as a system. At the ensuing return of the planet in 1879-80 they were again detected and drawn by him, with very little difference. But during the course of last January and February he has been so fortunate as to perceive the duplication of these dark streaks by the addition of parallel lines of similar character and length in no fewer than twenty instances, covering the equatorial region with a strange and mysterious network, to which there is nothing even remotely analogous on the earth, and which leads us at once to see how premature have been our conclusions in this respect, and how far we still are from any adequate conception of the real constitution even of our nearest neighbour but one in the solar system.

T. W. WEBB.

[I have thirty or forty tracings of views of Mars taken several years ago by Mr. Dawes—"eagle-eyed Dawes" as he was aptly named—in which, though he used but an 8-inch telescope, some of the long narrow passages mentioned by Mr. Webb are shown. I mention this, because it may serve to corroborate what otherwise might seem improbable, the circumstance that Signor Schiaparelli should have seen with his comparatively small telescope what has escaped the attention of observers using such instruments as the Herschelian reflectors, the three-feet reflector made by Mr. Common, and the magnificent 26-inch refractor of Washington. Albeit until observers with such instruments as these have distinctly seen what Signor Schiaparelli has mapped, we must not too hastily assume that these are real features of Mars. Mr. Nathaniel Green, whose fine lithographs of Mars adorn a recent volume of the Memoirs of the Astronomical Society, considers that these narrow passages are due to an optical illusion which he has himself experienced.

Should it be proved that the network of dark streaks has a real existence, we should by no means be forced to believe that Mars is a planet unlike our earth, but we might, perhaps, infer that engineering works on a much greater scale than any which exist on our globe have been carried on upon the surface of Mars. The smaller force of Martian gravity would suggest that such works would be conducted much more easily there than here, as I have elsewhere shown. It would be rash, however, at present to speculate in this way.—ED.]

WERE THE EGYPTIANS AWARE OF THE MOTION OF THE EARTH?

HAVING shown from their ancient hieroglyphical texts that the Egyptians understood the true motion of our planet, it now only remains for us to see whether this fact is corroborated by the accounts we have in classic authors of the opinions of the Egyptians on astronomical subjects. In discussing the matter from this point of view the greatest caution is necessary, because an ancient writer might assign to them an explanation of astronomical phenomena they never really held, from misapprehension, or supposing it is found in some philosopher's works who propounded a system allied to the Copernican, he might have falsely asserted his theory to have been derived from Egypt in order to gain for it a better hearing. M. Le Page Renouf, in his Hibbert Lectures, seems inclined to reject in many cases the assertions that certain Greek philosophers had been educated in Egypt, even doubting whether it was true that Pythagoras did so; but his views seem far too sweeping when compared with the universal testimony of the ancients, many of whose statements bear on the face of them evidence of truthful impartiality. For instance, Seneca says:—"Eudoxus first brought with him from Egypt into Greece a knowledge of the movements of the planets; nevertheless, he makes no mention of comets. Hence it follows that even the Egyptians, a people more curious than any other in all matters of astronomy, had occupied themselves but little with the study of these bodies. At a later period Conon, a most accurate observer, drew up a catalogue of the various eclipses of the sun observed by the Egyptians, but makes no mention of comets, which he would hardly have omitted if he had found any facts respecting them."

It would be an easy task to show that in all cases where the theories of the Greeks have approached most nearly to those we now know to be correct, they were expounded by men who are distinctly said to have studied in Egypt; but only a few of the most important of those which strictly appertain to our subject can be given.* Perhaps the most valuable remark of a Greek author is

* See Wilkinson "Ancient Egyptians," vol. II., 316, and I., 447. Also Humboldt, Cosmos II., 544, and 692, ed. Bohn. That Socrates *was* in Egypt is proved by a Greek inscription on an Egyptian temple, in which a Greek student says he was there "200 years after the divine Socrates."

XUM

to be found in Dio Laer, in the introduction to his "Lives of Philosophers," where, before describing them individually, he gives a short account of Egyptian doctrines derived from Manetho and the younger Hecateus, of Abdera. In this, after saying they consider the true form of God to be unknown, and that the world had a beginning, and is perishable, it says of the earth, "It is in the shape of a ball. The stars are fire, and the moon is eclipsed when it crosses the shadow of the earth." That we have here no mistaken statement of Egyptian thought set forth is certain from what Diodorus says, for he emphatically tells us that "the Egyptians knew that the earth was round," and that the words are to be taken literally is evident from his naïve comment, that were this true the Nile could not rise from the other hemisphere to flow into this. Whether it is fact that some Greek philosophers gained their knowledge in Egypt; the proofs that Pythagoras did so are convincing, and although it may be doubted we have any writings of his own, yet it is universally admitted that his disciple Philolaus is a correct exponent of them. In addition to the common testimony that his Pythagorean astronomical system came from Egypt, as was shown by Bökh in 1819, Wilkinson proves that his theories of transmigration, emanation, and music were all derived from the Nile Valley. Now Plutarch says: "Whilst others consider the earth immovable, the Pythagorean Philolaus believed that it moves around the central fire in an oblique circle in the same manner as the moon and the sun, and we have the evidence of Diogenes, Laertius, and Theophrastus, that Hicetas of Syracuse believed the earth revolved in a circle around its own axis. Philolaus here falls into the error of considering the central fire round which the earth revolves to be distinct from the sun, a mistake held, as Aristotle tells us, by the Italian followers of Pythagoras, but he may have been the cause of the error by misinterpreting the Egyptian views; and Aristarchus of Samos, a brother Pythagorean, has explained them correctly when he says, as quoted by Archimedes, who, after alluding to his denying that the earth was immovable in the centre of the universe, informs us he considered the sun constituted this centre, and was immovable, *like the other stars*, whilst the earth *revolved around the sun*. This great thinker alludes to the twofold motion of the earth on its axis and round the sun elsewhere; and further, the Egyptians, either from having observed the passing of sun spots over the solar disk, or for some other reason, conjectured that the sun also revolves, for, according to Plutarch, "In the Egyptian Hermetic books, when the sacred names are mentioned, Hermes is said to be Apollo, and to represent the *rotatory* motion of the sun" (Bunsen's "Egypt," vol. i., page 447). In conclusion, it may be said, therefore, that we have, as far as our knowledge enables us to judge, every reason to believe that the Egyptians understood the movement of the earth in the heavens. We arrive at this from two courses of investigation—from what may be termed the purely Egyptian, viz., authentic texts penned by themselves, and from the information handed down to us by the Greeks, mostly in reference to the doctrines of the Pythagoreans, which were, by everyone qualified to know, looked upon as Egyptian, but sometimes these correct ideas of astronomy are directly referred to Egypt herself.

A Member of the Society of Biblical Archæology.

MR. MATTIEU WILLIAMS ON COD "SOUNDS" AND "SCIENTIFIC PRIVILEGE."

I SHALL leave "Old Fossil" to settle with Mr. Mattieu Williams the nature of the structure in the cod, concerning the proper name of which Mr. Williams appears to be specially perturbed. I might remind the disputants that *post-mortem* appearances, both in men and fishes, are often deceptive. One thing, however, is quite certain—namely, that no one undertaking the dissection of a cod with any degree of care, could ever mistake the "sound" or "swimming bladder" for the *dorsal aorta*, or main arterial blood-vessel of the fish. The swimming-bladder of the cod contains air, not blood; although there is a beautiful network of blood-vessels (*retia mirabilia*) closely connected with the air-bladder. That which concerns me in Mr. Williams's communication is my former contention that whatever fishermen call the "sound," the term is used in zoology as synonymous with the names "air-bladder" and "swimming-bladder." As I do not find in Mr. Williams's article a single word disproving this contention, I must leave rhe matter where Mr. Williams's common sense is content to let it test. If he prefers, with the fishermen, to limit the name "sound" to another structure than the "air-bladder" he is very welcome to continue the practice. Zoologists are not likely to follow his lead.

A. Wilson.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For conveniences of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

THE POTATO.

[381]—I was pleased to see "Farmer's" contradiction in No. 20 to my article on the potato; for it shows me that English farmers are at last beginning to take some interest in agricultural research. May the day be not far distant when we shall be able to look up to our farmers as authorities on such matters, instead of having to look abroad to see how to feed our cattle, manure our land, &c., as we have to do at present.

I should, perhaps, have liked to have seen "Farmer's" letter a little more to the point; for as it now stands it is only "negative" in its contradictions, and I think I said in my article that "negative" results were of no use in science unless accompanied by "positive" (when obtainable), to bear out the investigator's or critic's assertions. For instance, "Farmer" should have told us the difference in weight between the Victorias from which the blossoms had been removed, and those on which the blossoms had been allowed to remain. Perhaps he did not think it worth while to give the weights, but they are really important in a matter of this kind.

"Farmer," I am sure, will excuse me for saying that I do not quite see the point of his second remarks; for he himself ends them by saying, "and has none of the sweetness peculiar to a *frosted* potato."

Thirdly. "Farmer" alludes to the compost; and as the "unmanured plot," &c., is connected with the same contradiction, I will answer the two by saying, that the "compost" was one of six or seven manures tried on the same kind of land (sandy, I believe), at the same experiment, while the "unmanured" was that plot which did not receive any manure; and so the deduction was fairly arrived at, I think. Has "Farmer" tried the effect of the different manures in this way? If he has, his results will be valuable. Further, I may say that in this case my remarks about "Potatoes grown," &c., do not exactly contradict the remarks about the "unmanured plots." "Farmer" says, "one assertion flatly contradicts the other, and both are contrary to his or any one else's experience." Does his experience, or that of his friends, lie evenly between the two points? He does not say!

I will now quote my authority for the various assertions I have made, and have much pleasure in doing so, especially if "Farmer" intends to pursue the subject in the paper I shall name, for I am sure another worker in the *Field* will be hailed with delight by all scientists abroad.

I will name his contradictions 1, 2, 3, 4, 5, to save space :—

1.	Bird.	Cents.	1879.	634.
2.	,,	,,	1880.	472-474.
3 & 4.	,,	,,	1879.	106-108. W. Paulson.
5.	,,	,,	1880.	868-870. W. H.

F. C. S.

TRICYCLES.

[382]—As a bicycle and tricycle rider of many years' standing, and one who has tried many kinds of tricycles, I would strongly warn your readers against taking Mr. Browning's advice, and getting a "creeping thing" in the way of a tricycle, with small wheels, under any circumstances. It is obvious that the smaller the wheels, the more must inequalities in the ground and stones inconvenience the rider, and this is an important point. Large wheels pass unjoltingly over an obstacle that would clash the teeth on a small 36-in. wheel.

In case of an emergency, I should not like to have my toes in a stirrup, as with the Monarch. It might be very awkward.

My settled opinion is, that a 50 or 52 in. Cheylesmore is the perfection of tricycles. The machine is beautifully finished, very light, and yet strong, and is also one of the fastest runners and easiest hill-climbers. Then comes the Rucker, the Premier, and perhaps the National. The Humber is fast, but I concur in Mr. Browning's remarks upon it in other respects.

It is a great mistake to have too heavy a machine, no matter the rider's weight, and strength can be secured without ponderosity.

I have just had a three days' ride to and along the salt coast and back on a Cheylesmore (about 190 miles), and rode every hill both up and down (although I crossed the South Downs), and experienced no fatigue whatever.

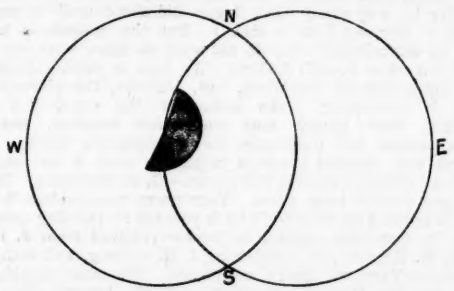
I fear the rotary Coventry is too narrow to be safe from overturning at a high speed.

The Cheylesmore is, I find, much less fatiguing on a long ride, infinitely more comfortable, and averages as fast a pace as a bicycle. Of this I am perfectly convinced from my experience.

I should like to dilate on the delights of tricycling and incidental matter, but I fear your space is too valuable to be afforded for the purpose. I should, however, be happy to give any practical information, should readers desire it.—Yours faithfully, Ex-Bicyclist.

HALO ROUND THE MOON.

[383]—On January 29, between 6.20 and 6.40 p.m., I saw a very peculiar halo round the moon. I was in Tokiyo (Yedo) at the time, and the same appearance was seen by many of my friends at Yokokama, 18 miles distant. It was like the figure herewith. At first I was under the impression that the figure was slightly elongated on the outer circle, that is to say, the distance from the centre of



the moon to the edge of the first halo was less than that from the edge of the first halo to the farthest point in the second; but on drawing it I am convinced that this was an optical illusion. The lower part of the second halo intersected the lower horn of the moon in a peculiar way, as given in the drawing. The moon was nearly half full.—Yours, &c.,

Yokohama, Japan, Feb. 13, 1882. H. Pryer.

PERSONAL ILLUSION.

[384]—I observe in your issue of March 18 that a correspondent, under the *nom-de-plume* of "Jumbo," mentions as an instance of the above the fact that to him "horizontal lines appear plainer than perpendicular" ones. I beg to suggest that this phenomenon more probably depends on a physical than a psychical cause. It is produced, in the great majority of cases, by a difference of refraction in the vertical and horizontal meridians of either the lens or cornea of the eye. The effect of this is to cause the one set of lines to be brought to a focus on the retina, while the ones at right angles to them are not. This condition can be corrected in most cases, to a great extent, if not entirely, by the use of cylindrical glasses. G. A. Herschell, M.D. Lond.

[Answered by W. R. D., F. T. W., and several others.—Ed.]

REPLIES TO QUERIES.

[318]—Vegetarian books and pamphlets are numerous, but perhaps G. A. S. would find what he wants in Dr. Nichol's "How to Live on Sixpence a-Day," price 6d. [Nichols & Co., 23, Oxford-street, London]. Of cheap cookery books there are, "How to Spend Sixpence," price 1d., "The Penny Vegetarian Cookery," and the "Food Reform Cookery Book," price 2d., to be had from the Secretary of the Vegetarian Society, 56, Peter-street, Manchester. I should be glad to answer any letters addressed 46, Meadow-street, Moss Side, Manchester, as a full answer to G. A. S.'s query would occupy more space than the editor could spare.—A Fellow of the Chemical Society.—[Many other replies received; but the above will suffice: and more also.—Ed.]

[337]—Self-Acting Blowpipe.—Such is for sale in the better class of tool-shops. A vessel with a screw top holding spirit, has a small pipe leading from near the top of its interior through the bottom, where it is turned at a right angle. A spirit-lamp underneath heats the spirit in the vessel; the vapour given off is expelled through the pipe, and passing over the flame of the lamp is ignited. A long roaring flame is the result, but it is not so hot as a good blowpipe flame, as the combustion is only that of the spirit without admixture of air. The flame is a large one, and is quite unsuited for soldering, but I have used it for manipulating glass tubing, as it allows the use of both hands.—Clare.

Answers to Correspondents.

*** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

ASTRONOMY AND MATHEMATICS.

J. Murray. How severe the P. D. system must be, which will not allow Venus to be more than 5,000 miles in diameter! It is some comfort to know that, as "gravitation or retardation is a compound body, it is hard to say to a 1,000 miles off-handed." Yet you say, "Without a dought" (the worst spell of doubt I have yet experienced) Mars is only about 1,600 miles in diameter.—A. N. S. (1) Thanks for calling my attention to the undoubted mistakes occurring in the times noted p. 468. I took down the *Nautical Almanack* for 1881, instead of 1882. Fortunately the reasoning is in no sense affected by the mistake. (2) Astronomers find it convenient to take the sun's centre; that is the only reason I can think of.—D. Maxwell. Your reasoning is incorrect, though plausible. You cannot so simply deal with the attraction of the segment of a sphere, as to assume that it acts as though the whole mass were at the centre of gravity. The sphere (either of uniform density, or composed of concentric shells, each of uniform density) is the only solid whose resultant attraction on a particle is the same as though its whole mass were collected at the centre of gravity. As a matter of fact, the force of gravity within a globe of uniform density diminishes as the distance from the centre. Note also, that if through a particle in such a globe, a spherical surface concentric with the surface of the sphere be supposed to pass, the portion of the globe outside that inner spherical surface produces no effect whatever on the particle.—M. Boyce. Your friend was right. The sun is far north of east at 6 A.M., either mean or solar time. By solar time, he crosses at 6 A.M. and 6 P.M. the great circle passing through the east and west points and the pole of the heavens (a declination circle), *not* the great circle passing through the east and west points and the zenith.—A. S. T., M. H. C., Erin-go-Bragh. Thanks.—Noblehill. I beg to inform you that pressure would not vanish at the earth's centre. Pressure *there* is not the result of gravity *there*, but of gravity *elsewhere*, and therefore your reasoning has no weight. The matter above the earth's centre has, on the contrary, a great deal of

weight—hence, very much pressure.—R. Bailey Walker, F.S.S. Many thanks.—F. H. J. H. You can magnify a flea to the size of a brewer's horse, or of Jumbo, by means of the oxyhydrogen lantern. —Senex. Certainly, the velocity and distance of any two planets are connected by Kepler's law. Thus, take two planets A and B, let their periods be p and P, their distances d and D, their velocities v and V. By Kepler's law:—

$$d^3 : D^3 :: p^2 : P^2$$

$$\therefore \frac{d^3}{p^2} = \frac{D^3}{P^2} \cdot \frac{D}{d}$$

$$\text{But obviously } v : V :: \frac{d}{p} : \frac{D}{P}$$

$$\therefore v^2 : V^2 :: \frac{d^2}{p^2} : \frac{D^2}{P^2} :: \frac{D^2}{P^2} \cdot \frac{D}{d} : \frac{D^2}{P^2}$$

$$:: D : d$$

whence $v : V :: \sqrt{D} : \sqrt{d}$, or the velocities of planets are inversely as the square roots of the distances.—J. Lankshear. Yes, the darker part of the moon can often be seen. That part is lighted by the earth; for when the moon is new to us the earth is full to the moon.—C. E. Thorburn. Thanks.—J. T. D. How could the pole and centre coincide, however the earth might be flattened? If they coincided, the pancake earth would be of no thickness at all, which is absurd. Centrifugal force at equator accounts for only a part of the difference. For the attractions at equator and pole, independently of rotation, see Todhunter's "Statics." The moon question rather more complex than you seem to think.—J. B. Moffat. We have not "G. E. V.'s" address.—F. D. It seems to me there is no confusion and no difficulty in the probabilities question.

ELECTRICAL.

Eccentric Chuck.—1. Swan's lamps are 5s. each, the other forms are 10s. to 12s. 6d. 2. About twenty Bunsen cells, in good condition, will light up a Swan lamp for three or four hours at a time. 3. Speaking at the Royal Institution a few weeks since, Mr. Swan said his lamps were reliable for 1,200 hours continuous burning; but I believe they are now safe for at least 2,000 hours, or about twelve months ordinary burning.—L. J. The Swan lamp (see preceding answer) may be obtained for 5s. of Swan's Electric Light Company (Limited), 13, Mosley-street, Newcastle-on-Tyne; 25s. was the price, until recently, and further reductions, at least, in the other systems may, perhaps, be expected.—Ashbridge-Robinson. A large-sized Pickford van might, perhaps, hold a sufficient number of Daniell Battery cells to maintain fifteen Swan or Edison lamps, but it is very doubtful. An induction coil is of no use. You would require an engine and a dynamo-electric machine.—A. W. B. To make a Grove's cell, get an earthenware jar of, say, a quart capacity, into which put an amalgamated zinc cylinder, and inside this a porous pot containing a sheet of platinum. In charging, put concentrated nitric acid in the porous pot, and sulphuric acid, diluted with ten times its volume of water, in the outer cell. Substituting a block of gas carbon (a waste product in gas-making) for the platinum converts the Grove into a Bunsen. Generally the Grove is made in the flat form, the Bunsen almost invariably being round. Is this lucid enough? You can buy either for about 5s. per cell, but can make them for about 3s. or 3s. 6d. 2. Your best plan is to write to the various companies, offering your services, and stating your qualifications.—E. C. H. A description of the Brush machine is already written, and only waits its turn to appear in Knowledge.—S. G. T. Really, I am puzzled how to answer your queries. However, I will try what I can do. In the first place, the engine does not produce electric sparks. Secondly, friction is of no use to produce the electric light; and, thirdly, galvanic batteries are not very much better, although a light can be obtained from them; nor, fourthly, are the "magnetic coils affected by the action of the engine" beyond being rapidly rotated before the poles of a magnet. Read our articles on "Electric Generators." "The Engineer," and "Engineering," for the past two or three years, contain the information you require about gas-engines.—F. A. S. We shall describe the gramme machine "when space permits," but it is more difficult for amateurs to make than a modification of Siemens's machine, which we are describing in our columns for the "Amateur Electrician." Urquhart's "Electric Light" is, I think, the most recent work on the subject; but, although a good book, it is rather crude in some of its descriptions.

MISCELLANEOUS.

G. M. Gorham. The true theory of the rainbow would be rather too abstruse for our readers; albeit, if you had not lost the one you mention, we should be very willing to publish it.—M. T. H. Do you therefore reason that because our grandmothers were idiots in matters of dress, their granddaughters need not mind being merely foolish?—F. H. S. Your questions quite unsuitable. Our readers (and we too) care for none of these things.—Fons Bandusii. Do not know.—Gregory. Very likely there is a good deal of electricity about the sun. Any details?—P. H. Carpenter. We have already arranged with a geologist for the discussion of such subjects. *Our Query column is defunct, and so is our Reply column.*—C. R. Bree, M.D. Yes, but unfortunately for your argument the egg is not an ellipsoid.—W. Mabel. We cannot take up the subject. If the trade cares to try it, let them do so; they have our full and free permission. They have not asked for it, but that is a detail.—J. Allen Brown. Quite so. Dr. Siemens's theory will, however, never be attacked seriously by science, for a very sufficient reason—it has no life, therefore needs no killing.—Anno Domini. "Oh, bosh! the worthy bishop said;" for which you will overhaul the "Bab Ballads"—perhaps.—G. W. B. Yes, but perhaps Mr. S——, of London, will advertise as much for himself. It is not our business to do so.—Bernard Batigan. See No. 24.—W. Roberts. Arabic names explained shortly. Know nothing showing Cromwell believed in astrology. He was superstitious in some things—strangely so for so strong a mind.—J. A. Bromley. Many thanks for the Dutch barometer. My being the "fountain of knowledge" does not help me to get the water in, but will try all three given methods.—Thomas Smith, Jun. Nobody questions that character is shown by the shape of the head; so it is shown hy the chin, the nose, the cheek-bones. The so-called science of phrenology, as advanced by Gall, Spurzheim, and all their followers who really know what phrenology means, asserts that underneath the so-called bumps are the cerebral organs corresponding to the several qualities associated with those bumps. It is this that science rejects. One can tell character as well from nose, lips, chin, eyes, jaws, and so forth, as from cranial development, and nine-tenths among so-called phrenologists (I suspect all) really judge of character thus, and not from the bumps. Now I venture to assert that there is as much reason for asserting that there is an organ of Voluptuousness (Capital V., if you please, Monsieur le Compositor) within the lips, and of Resolution within the chin, because full lips indicate a voluptuous character and a prominent chin resolution, as for saying that there is an organ of Destructiveness in the part of the brain behind the ears, and an organ of Philoprogenitiveness just above the nape of the neck. But the fact of the matter really is that so-called phrenologists of our time know nothing whatever of what Gall and Spurzheim really taught.—J. I never met with a man of science yet who did not agree with what, as you tell me, Professor Boyd Dawkins said at Manchester. Sir W. Thomson's theory of the meteoric origin of life is a joke, nothing more; if he really maintained it, it would be a jest.—F. W. Halfpenny. Yes, there can be no doubt the Egyptian books contained nearly all that we find in the books you mention, and a good deal more; so did the Assyrian stone records: and many go about saying, in favour of the account you refer to, very much what Rogue Riderhood used to say for himself ("*Now* say I'm a liar.") But the subject is hardly suited for our columns. We do not want to know what has been taught, but what *is*.—W. J. Cant. We hope to publish the figures drawn by compound pendulum, but, hitherto, the photographic record is incomplete. The author of the paper you mention has very kindly sent some red tracings, but the red is aniline, and unsuitable for photographic effect. Your stamped and directed envelope might be used if we could do so without injustice to some 750 others.—G. H. Mortimer. We can no longer publish book titles. Your query unsuitable.—E. C. R. Can you not look up Mr. Judd's book yourself? (Similar questions —that is, questions relating to books—received from J. H., L. Brown, W. Hartwig, M. Conybeare, J. H. Ludwig, and multitudes of others.—Vacuum. Query unsuitable. We must consider the many, not the few, or mere units.—M. B. Alder. You think Grant Allen very wicked, and also very silly, because he says, "the daffodil has done so and so;" others (myself, for example) think his way of putting these things perfectly delightful. What can I do, except to quote the old saying, *De gustibus non est disputandum?* But, tell me, where does science stop and blasphemy begin? Is it blasphemous to say that child grows, or this tree thrives? If you can allow your mind to admit development on the small scale, can you not, by any possibility, admit it on the larger scale too? Can you not see that to Him "who works in and through all things," there can be no distinction (as with us) of large and small, long lasting and transitory? For my own part, when I hear fanatics raving against the general doctrine of evolution, I see in their ineptitude the strongest evidence for evolution. Their inability to see that the same Being who can arrange for the evolution of the fowl from the egg or the man from the embryon can also presumably arrange for the development of the race also from a race of lower

type, may be compared to the incapacity of the infant for that which is easy for the grown man, or for the inability of the lower races to effect what is well within the power of the higher. I say with you, "there is something loathsome about untruth." It would be exceedingly loathsome, therefore, for me to say I agree with you in aught save that general proposition—Narcissus, Lunatic. "No more on that head," as the bald man said.—Foreigner. Man is inquisitive, and wants to know what there is at the Pole. That is the only reason I can imagine for polar expeditions.—Easy. Thanks; but do not see our way to taking a part in the matter.—Scalpel, C. H. C. We have been obliged to decline to insert *any* question asking for name or names of books on special subjects, though we question the *bona fides* of no individual querist. In three cases we have had replies to such queries in the same handwriting as the queries themselves. We are sorry that for the sake of a few dishonest querists, the great majority of honest ones suffer, but we cannot help it.—Nicholas Whitley. That may do very well for the V. I., but we would as soon insert a paper by Parallax asserting the earth's flatness, or any other absurdity.—J. B. T. Well: but that is just what inertia requires.—R. W. Branthwaite. You say "the dental formula at p. 459 is entirely wrong and needs correction," and then you give the dental formula for man. Since No. 1 of Knowledge appeared, I have been told many things which I knew before, and I thought scarcely anything in that way could surprise me; but I frankly confess you beat me. I did not expect to have it carefully (and I must admit very lucidly and fully) explained to me that man does not ordinarily possess twelve incisors, or twenty-eight molars, or forty-four teeth all told. "It will go near to be thought so shortly." Where, in p. 459, is it stated that the dental formula there given is man's? Rob Croniel (? ?). We could scarcely find space for Cottier Life. —Gerald Massey. Thanks; but these verbal questions are not for us. The mistakes in the letter arose from its accidentally escaping correction. There are no such eight stars.—G. W. Hart. Thanks; but Chemistry of Genesis would hardly suit us.—Thos. A. Cotton. We cannot say where that or any instruments are to be purchased, either here or privately. In fact, stamped and addressed envelopes, except for return of MSS., are sent to us in vain. Our rule on that subject has been very clearly stated.—C. A. R. The subject is a good one, and we hope to have such an article shortly.—G. F. Hanap. We cannot take the office of private tutor even to a section of our readers. Besides, the questions you ask are not even complete.—James Deas. Guillemin on Comets, translated by Glaisher. Can recommend no book on astrology.—Unitas. Thanks. —Cogito. Mr. Grant Allen appears to presuppose some intelligence in his readers, when he speaks as if there were intelligence and volition in plants.—J. Russell C. Cheese problem rather too ordinary. —Alpha. The right answer, as I understand the question, is 42⅔. A takes one-third before D, who takes two-ninths, retires. A then should now take nine-sevenths of one-third, or 42⅔ per cent.—Arthur Gearing. You solve a problem not given in these pages. A rectangle formed of three squares is not the same thing as three squares arranged so as to form three-quarters of a square.—Zeta. Loomis's Astronomy has good refraction tables. Price, I think, 8s. 6d.—A. N. S. If that (referring to your probabilities question) needs proof, we had better prove that two straight lines cannot enclose a space.—H. C. Standage. Thanks, but table of payments quite unsuitable.—Comitatus. I suppose the front engine draws the front half of the train, and the hind engine pushes the hinder half.—J. Parker Fowler. If you were acquainted with the formulas of spherical trigonometry you could hardly ask the question; if you are not acquainted with it, we can hardly answer it. Why cannot we see through a brick wall? sounds like a conundrum. Light is not "lost in passing through the wall." It does not pass through, but is partly reflected, partly absorbed, at the surface.—James Harrington. I do not know Mr. John Hampden's address, but if I did, I could not forward your communication; it would be a breach of our rules. Let me advise you not to waste time and money on such nonsense.—Erin-go-Bragh. Thanks; but the facts mostly familiar.—H. C. Standage. Pigments not in our line.—S. M. Cox. Thanks. About possible hands there is no difficulty so long as there is clear definition of what constitutes different hands. —B.M., F.R.C.S. You may depend upon it Sir Edmund Beckett knows all about the advantage of having the screw-driver properly ground. Telling *him* that a mechanical device he suggested would be troublesome, unworkmanlike, and unnecessary — would be like telling Hanlan that he had entirely erroneous ideas about rowing.—M. Wyatt. Thanks; but as there is no practical value in rules for finding roots, we must reserve the space the subject would take for other and more pressing matter.—An Engineer. We noticed the passage, but did not understand it as you have done. No one can for a moment suppose that the presence of air in a bird's bones could add to the bird's buoyancy; but it should not be necessary to explain to an engineer that the hollow structure of the bones is adapted to give as much strength with as little weight as possible. The point you explain is fully dealt with in a Fitful reply of my own at p. 60.

Letters Received.

Bobby.—(You are cool enough). J. Hargreave, L. M. N., K. Sperritt, M. N., J. Hannay, F. Finleyson, R. Morris, S. T. P., K. Pavitt, M. R. Winstanley, Peter Parley, Post Meridian, F. Friendlison, Avernus, M. J. Merryweather, Semper Paratus, R. Rudolfsen, Mons Parturiens, S. Silliman, J. R. Rendell, M. Purvis, K. H. Payne, R. P. T., Rap Back (Why? R. A. P.), Per aspera ad ardua tendo (Many do), J. V. M., Holocaust.

Our Whist Column.

By "Five of Clubs."

Play Second Hand when Knave is Led.

KNAVE, as an original lead, is played only (see synopsis of leads, p. 310, No. 14), (1) from King, Queen, Knave, and two or more small ones; and (2) from Knave, ten, nine, with or without small ones; except in the unusual case that original leader holds four trumps and three three-card suits, when Knave may be led from Knave, ten, and another. If then second player holds either King or Queen, he knows the lead is not led from (1); while, if he holds either ten or nine, he knows it is not from (2); and, lastly, if he holds one of the first set, as well as one of the second set, he knows that the lead is a forced one, the leader having probably no four-card plain suit. Taking the two more common cases, of a Knave lead, second player, if he holds King or Queen only, above the Knave, showing that the lead is from Knave, ten, nine, and others, should play a small one, unless with the Queen he holds the ten, when he should play the Queen. But this last case is very unlikely to occur, as it would imply that Knave had been led from Knave and one or two small ones. If second player holds both King and Queen, he should cover Knave with Queen. If he holds Ace and Queen with or without small ones, he should play the Ace, knowing that leader does not hold the King, so that covering with Queen is useless, whether third player or partner hold the King. If, when Knave is led, second player has no card above it, he will, of course, play his lowest. If second player holds King, nine, or Queen, nine, the lead is probably a forced one—from Knave, ten, and a small one; it is, therefore, useless to put on the higher card, unless circumstances render it very desirable to gain the lead. But usually the small card would be played, leaving partner to take the trick if he can.

Play Second Hand when Ten or Nine is Led.

Here the principles are much the same as in the previous case. Ten (see synopsis of leads) is only led from King, Queen, Knave, ten, and from King, Knave, ten, with or without others. Therefore, if second player holds the Queen, with nothing to show that the lead is forced (and ten is very unusual as a forced lead), he knows that the lead is from King, Knave, ten, and should play the Queen, or not, according as she is singly or doubly guarded. It is obviously useless to retain her if she is only singly guarded, for she must fall next round; and as obviously unwise to play her if she has two guards or more. If ten is led, and second player holds Knave and nine (a fourchette), of course Knave should be played. The lead, with these cards outside the leader's hand, is very unusual, being from ten and one or two small ones. Nine is only led from King, Knave, ten, nine, with or without others. If second player, then, holds Queen only singly guarded, he should play her; but if she is doubly or trebly guarded, he should play his lowest.

Play Second Hand when Small Card is Led.

The rules for play second hand when a small card is led are nearly all included in the general instructions already given. When second player is long in the suit led, he plays somewhat as he would if the suit were his own, and he were leading, only that, first, in playing one of a sequence he always plays the lowest, and, secondly, as his partner is in a more favourable position, being fourth player instead of third, he leaves more open to him. For instance, in leading from Ace and four small ones, Ace is played; but if second player holds these cards he plays a small one, unless the game is in a critical state, and either one trick or a lead is much needed; or if first player is one of those who affects the lead from a singleton. So when an honour is led and you hold a

XUM

higher honour and are long in the suit, you pass the trick, except you have strong reason for desiring a lead (or wish to stop a trump lead, but that case belongs to play second hand in trumps). When, however, you hold Ace, Queen, and others second hand, a small card being led, your play is unlike that which you would adopt in leading from such a hand. Thus, with Ace, Queen, Knave, with or without others, Ace is always led, but with these cards second in hand, you play the Knave (or lowest of Queen Knave sequence); from Ace, Queen, ten, Queen is to be played; for if third player covers you remain with the tenace, so that unless led through again, and Knave lies with third player, you are sure to make two tricks in the suit (apart from ruffing). From Ace, Queen, and one or two small ones, you play second hand a small one on a small card led. From Ace, Queen, and three small ones or more, you play Queen, if weak in trumps, smallest if strong.

When you are short in the suit led, you play second-hand on somewhat the same principles as in the case of the forced lead *of a suit in which you have no reason to believe your partner strong.* You play, of course, the lowest instead of the highest of a sequence, and you leave more to your partner, as he is more favourably placed.

But if you hold only two cards in the suit led, you reverse, as second player, the rule which holds, almost without exception, in the unusual case of a forced lead from a two-card suit. Thus, if you hold King, or Queen, or Knave, and one small one, you play the small one—not the honour (in plain suits), unless you have some special reason for desiring to obtain the lead, in which case the King may be played, second hand from King one small one. Many skilful players of their own hand maintain, with some obstinacy, that Queen should be played second hand from Queen one small one, because the chance of her falling is so great, greater than that of King falling when you retain him second in hand. The chance of a singly guarded Queen making is always much less than that for a singly guarded King; but you do not increase it by playing the Queen in such a case. You know certainly that either King or Ace lies with third and fourth players. If third holds either, your Queen falls. It may be, third player puts on King, and your partner takes the trick with the Ace, in which case you have the satisfaction of knowing that you have thrown away your Queen. Of course, if your partner holds Ace, and third player does not hold a King, you gain by putting on the Queen. But this is practically the only case in your favour. The matter may be put thus: One of the following arrangements *must* hold; as it is certain, from the lead of a small one, that leader has not both Ace and King:—

	By playing Queen,
3rd holds Ace and King	... You throw her away
4th „ „	... You save a trick
3rd „ „ 4th holds King or not	... You throw her away
3rd holds King, 4th holds Ace	... You lose a trick
„ „ „ not Ace	... You throw her away
3rd weak, 4th holds Ace	... You gain a trick
„ „ „ King	... You probably gain a trick.

There is, you observe, a balance in favour of the play of Queen second-hand turning out unfavourably.

Cavendish gives a reason for playing the small card, the force of which some whist players seem not to recognise. He says that, playing the Queen unnecessarily exposes your weakness, and enables the leader to finesse when the suit is returned. As the Queen is the proper card to play if you hold King, Queen, and a small one, you expose your weakness to the player who holds King. If this is the leader, he can finesse the ten if he holds it or his partner leads it, on the return of the suit, knowing you do not hold the Knave. He may even finesse the nine if it suits him; a finesse against only one card. If the third player holds the King, he puts it on, and your weakness is exposed to both adversaries, who can finesse under equally favourable conditions. Of course, if fourth player holds the King, you only disclose your weakness to him; but that is one favourable case to two unfavourable cases. Manifestly, the play of Queen second hand from Queen one small one is unsound.

Next week we shall give, with remarks, inferences on a game singularly well illustrating the weakness of the lead from a singleton, even when the object is to secure only the odd trick, so that there is no question of bringing in a long suit.

G. Thompson.—Solution of Problem 2 quite right.

Solutions of Problem 3, by G. Nielroy, Moleque, D. Maxwell, W. R. Flett, F. D. Mackenzie, Wm. R. F., J. Harvey, M. Purvis (or Provis), J. Pocklington, P. R. M., correct. All others wrong. There is no way of making all the tricks unless the Queen and Knave of Spades in *A*'s hand are discarded.

Unitas.—Pole's "Whist" published by Longmans. Clay's by De la Rue & Co. Our latest edition of Cavendish is the twelfth.

SOLUTION OF PROBLEM 3.

	A.	*Y.*	*B.*	*Z.*
1st Trick	S Ace	S 2	S 7	S K
2nd „	H Kn.	H 7	H 3	H 2
3rd „	H 6	H 8	H 9	H 5
4th „	S Kn.	H K	H Ace	D 4
5th „	Qn.	D 2	H 4	D 7
6th „	C 5	S 3	S 10	D 8
7th „	3	S 4	S 9	C Kn.
8th „	C 2	S 5	S 8	C Qn.
9th „	D 5	C 4	H Qn.	C K
10th „	D 6		H 10	D 9*
11th „	D Kn.	C 7	D 3	D 10
12th „	D Ace	C 8	C 6	D K
13th „	D Qn.	C 9	C 10	C Ace

The lead of Spade Ace, and the discard of Queen and Knave, are the "points" of this problem.—Five of Clubs.

Our Chess Column.

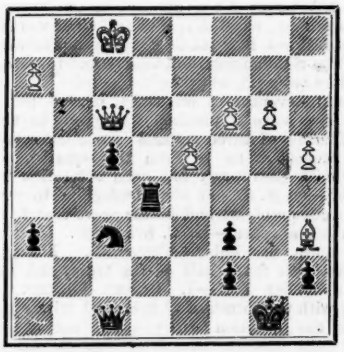
GAME BY CORRESPONDENCE.—(*Continued from p.* 505.)

Position after Black's 35th move.

R takes R.

CHIEF EDITOR.

WHITE.



BLACK.

CHESS EDITOR.

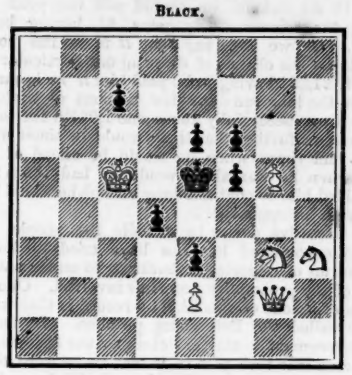
36. Q takes QBP	36. R to K8ch
37. K takes R	37. Q to Ksq ch
38. Q takes Q	38. Kt takes Q
39. K to B2	39. Kt to B3
40. K to B3	40. Kt to Q4
41. B to B4	41. Kt takes P
42. K takes P	42. P to QR4
43. K to B5	43. Kt to Kt 8
44. P to KR4	44. Kt to Q7
45. B to Q5	45. Kt to B8
46. K to Kt6	46. Kt to K6

* If *Z* here plays his C Ace, *B*'s C 10 is then best C, and he plays it before D 3, the result being the same.

PROBLEM No. 35.

By Leonard P. Rees.

Black.

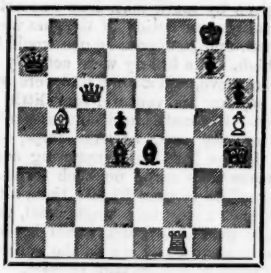


White.

White to play, and mate in two moves.

PROBLEM No. 36. By Ercole Del Rio.

Black.



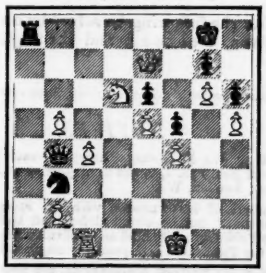
White.

White to play and mate in four moves.

PROBLEM No. 37. Example of "Smothered Mate."

(Black received the odds of Queen's Rook.)

Black (Mons. Gretener).



White (A. J. Maas).

White's last move was Q from KR4 to K7. Black then played Kt takes R. White now mates in five moves, beginning Q takes KPch. (If White checks at QB7, followed by Kt K8, Black can draw by perpetual check.

THE GIUOCO PIANO (*Continued from p. 442.*)

1. P to K4 / P to K4 2. Kt to KB3 / Kt to QB3 3. B to B4 / B to B4 4. P to B3 / Kt to B3

Besides 5P to Q3 and 5P to Q4, which we have examined in our last article, White has four other moves at his disposal, viz., 5Kt to Kt5, 5 P to QKt4, 5 Q to Kt3, and 5 Castles. 5 Kt to Kt5 is the weakest of these continuations, but as it presents some danger to the inexperienced, we will examine it first :—

5. Kt to Kt5 / Castles. 6. P to Q3 / P to KR3 7. P to KR4

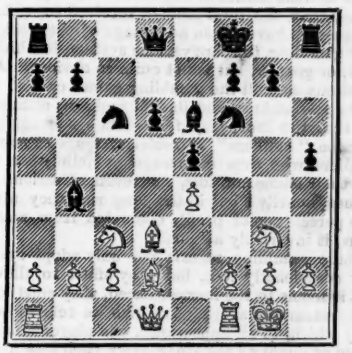
This is a position which has occurred in thousands of games. The danger arises if Black should now capture the Knight with his Pawn, then White would win by having the command of his whole King Rook's file, *i.e.*,

7. ——— / P takes Kt 8. P takes P / Kt to R2 9. Q to R5 / R to Ksq 10. Q takes BPch / K to Rsq 11. R takes Ktch / K takes R 12. Q to R5 / Mate.

But, of course, Black would do well to play ——— / 7 P to Q3, and he will soon obtain a good game. Should White play 8 Q to B3 then ——— / 8 Q to K2, to be followed either by B to K3, or Kt to Kt5, in which latter case Black might possibly threaten to capture the Knight, provided the Queen cannot enter either on R5 or R3. This latter contingency will ultimately force the retreat of the White Knight from Kt5, again leaving Black with a superior position. Black might also play ——— / 6 P to Q3 at once, instead of ——— / 6 P to R3, and he will also have a good defence. The danger of this manœuvre is by no means confined to this particular opening. As a very remarkable instance we here give the ending of a game which occurred in the Berlin International Tournament of 1881, between Herren Zukertort and Shallopp. The latter, as second player, adopted the Sicilian defence.

Position after White's 13th move (Castles).

Black.



White.

White (Zukertort).	Black (Shallopp).
Herr Shallopp proceeded with :—	
	13. Kt to Kt5
14. P to KR3	14. Q to R5
15. P takes Kt	15. P takes P
16. R to Ksq	16. Kt to Q5

and White resigned, as after this move he had no defence. Black threatens Q to R7ch and Q to R8mate. If 17. QKt. to K2, with the object of interposing that Knight on Ktsq., to prevent the mate, then Black would reply with 17. Kt to B6ch. 18. P takes Kt. 18. P takes P, and again it is impossible to prevent the mate.

But in spite of this wonderful success, we have no hesitation in declaring this attack unsound, for had White delayed the capture of the Knight for a few moves only he might have played so as to provide protection for his King, which White thought was available after P takes Kt, overlooking Black's clever resource of Kt to Q5, which prevents the King escaping by way of K2. Thus, for instance, after

	13. Kt to Kt5
14. P. to KR3	14. Q. to R5

instead of taking the Knight at once he ought to have played

15. QKt to K2	15. B to QB4
16. P takes Kt.	16. P takes P

17. R to Ksq winning,

as after 16. Q to R7ch, 17. K to Bsq, White now has a safe game, besides being a piece ahead.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Correct solutions received from Muzio, G. W., A. McDonnell, E. J. Winterwood.

C. P. and Henry Selby.—See revised problem.

E. A. Dillon.—Received with thanks.

XUM

Our Mathematical Column.

PROBABILITIES.

By the Editor.

LET us next deal with some cases not altogether so simple as those hitherto considered.

Suppose that there is a lottery, in which there are more prizes than one, the prizes being unequal in value—how can we determine the value of a ticket?

Take a simple but definite case:—

Suppose there are ten tickets, all equally likely to be drawn, and that there are three prizes, worth respectively £6, £3, *and* £1, *what is the value of a single ticket?*

The prizes are together worth £10. It follows that the ten tickets must together be worth the three prizes together, for any one buying all the tickets would get all the prizes and no more. Hence the ten tickets must together be worth £10, or (since they are all equal in value) must be worth £1, just the same price as when there is a single prize of £10.

And manifestly this is so in every case. It matters not how the prizes are distributed, the value of one among n tickets is always one nth part of the total value of the prizes.

It is probably this simplicity in lotteries of this kind, and the consequent obvious nature of the fraud when the total value of the prizes is less than the total amount received for tickets, which has caused those who have taken advantage of the weakness of human nature for gambling, to adopt various systems in which the swindling is as great, or greater, but is not quite so obvious. All the existing lottery systems, and all the gambling games carried on formerly at such places as Homburg and Baden, and now at San Marco, are so arranged that the luck may for a while run against the lottery holders or the "bankers" at roulette, rouge et noir, and the rest. The swindlers who thus encourage gambling can truly say that they take their chance of loss, and even of serious loss. They do lose at times, heavily; but in the long run they always come out right, the percentage of profit, estimated from mathematical considerations, is invariably attained.

Nay, these gambling rascals not only adopt systems by which they may occasionally lose, but they affect to allow privileges by which, as it seems to the inexperienced, they *must* lose. They allow that very system of wagering to which we referred some time since as one by which, to all appearance, one player must always win,—the system of doubling the stake after each loss until finally a win leaves a balance of gain as against several previous losses.

Let us take the simple case stated by us before, and see where the fallacy about sure gain lies:—

A *tosses a coin with* B, *staking* £1; *if he loses he stakes* £2; *if he loses he stakes* £4; *and so on, doubling each time until he wins, when he clears* £1. *And, as he must at length win, he can keep on adding pound to pound,* ad infinitum, *yet each separate wager is fair. Where is the fallacy?*

The fallacy resides in the supposition that A must at length win. He may go on doubling till he no longer possesses enough money to wager again on this doubling system, or till, having wagered more than he possessed, he is unable to pay. He is then ruined, and the process of adding pound to pound comes, perforce, to an end. In the long run, if B only has money enough as compared with A, this untoward event is bound to happen. If A and B have nearly equal capitals at starting the case is in some degree different: A may ruin B. But in the case of the bank at San Marco, or wherever else gambling may be pursued (of course mere coin-tossing is not the method but only illustrates the method), there are multitudes of small A's all risking their "small peculiar" against the possessions of one big (and very busy) B. They are inevitably absorbed separately if they are only possessed strongly enough by the gambling spirit.

Let us see what are the respective positions of A and B with regard to the prizes actually at stake in this case:—

At each venture A plays for £1, and we may consider that B stakes £1. If A's capital allows him to go on doubling ten times before he gives in, B plays for what A will have to pay him if he, A, is obliged to stop. The amount will be the sum of A's successive payments up to, and inclusive of, the tenth doubling, or

$$£1 + £2 + £4 + £8 + \ldots\ldots + £256 + £512 = £1{,}023.$$

The sum of the prizes is therefore £1,024; and there are 1,024 possible events, for there are 2 possible events at each tossing, and, therefore, 2^{10} possible events in 10 tossings. Therefore, at each venture (not at each tossing, but at each setting-off upon a series of tossings with constantly doubled stakes) B is practically in the position of one holding a ticket in a lottery of 1,024 tickets, each priced at £1, and a single prize of £1,024 [for note that we must not call the prize £1,023, any more than in the case of a fair lottery of 10 £1 tickets, we should call the prize £9, because that is all the winner really gains, £1 having been paid for his ticket]. Or we may say that B is in the position of one who pays £1 for the chance of drawing one particular ball out of a bag of 1,024, £1,024 having to be paid him if he is successful. We know that in the long run—in a few millions of trials, for instance—he would draw successfully about one 1,024th the total number of trials. We know further that there would be times when he would be behind, and times when he would be ahead of this average. The times when he was ahead would be bad times for A. If B's capital enabled him to continue long enough he would be practically certain to ruin his opponent.

In their excessive desire to swindle the people, Governments which have encouraged lotteries have tried to devise the most attractive forms of wagering, submitting to mathematicians the due discussion of the probability problems involved. One of the most remarkable occasions of this kind on record is that which gave rise to what is called the Petersburg problem. It occurred to the Russian Government to start a lottery on the following plan:—

Each person who took part in it was to venture the same fixed sum of money £x, on the following conditions:—A coin is to be tossed until head appears; if head comes at the first toss the person is to receive £2; if at the second toss the person is to receive £4; if at the third toss, he is to receive £8; if at the fourth £16, and so on. The difficulty was to determine at what amount should x be fixed?

The answer given by mathematicians was not encouraging. We can form a tolerably clear notion of the sum we should care to risk on such a venture; and I suppose no one would be inclined to place that sum very high. If a lottery were actually established on such a principle as the above, and £10 or £12 were set as the value each person was to pay for his venture, very little business would be done at the price. But mathematicians asserted that if the value of x were set at *any sum, however large,* the "bank" would inevitably lose in the long run, supposing only that a sufficient number of ventures were made on such terms. For example, say the value assigned to x were £1,000, then, although it would be madness to risk such a sum on a single trial, yet if many millions of ventures were made, the bank would be immensely in arrears when a balance came to be drawn on the results of all the trials. The theoretical value of x is, in fact, *infinity*.

This curious paradox is described by Professor De Morgan as affording one of the most instructive lessons on the subject of probabilities.

I shall explain hereafter the reasoning by which the above seemingly paradoxical, but undoubtedly true, answer is obtained.

Problem.—Can the following be solved by elementary geometry? In a triangle given, $a+b$, $a+c$, and the angle A to construct the triangle?

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